ORGANIZATION OF LIFE

ELDRIDGE

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A REVALUATION OF EVIDENCE RELATIVE TO THE PRIMARY FACTORS IN THE ACTIVITY AND EVOLUTION OF LIVING ORGANISMS, INCLUDING A FACTORIAL ANALYSIS OF HUMAN BEHAVIOR AND EXPERIENCE

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To
MY WIFE
RUTH ELDRIDGE

PREFACE

Scientific inquiry embraces the collection of data on unsolved problems, and the analysis of such data with a view to arriving at correct conclusions on those problems. These two phases of scientific method are inextricably interwoven, and are indeed but different aspects of a systematic procedure whereby problems are solved, and human knowledge extended. Nevertheless, one or the other phase of this procedure may be emphasized in particular investigations or in the work of individual investigators; and investigators who are equally adept at collecting and analyzing scientific data are somewhat rare. This is no disadvantage, and may be a positive advantage, provided that, for particular sciences and for science as a whole, a proper balance between the two processes is maintained.

There is a tendency, as methods of observation and experiment are developed, to emphasize the collection of data, and to underestimate the value of a thorough analysis of the data collected. The latter is not and could not be dispensed with, for every scientific experiment or observation is designed to throw light on a situation which had been previously subjected to a more or less thorough analysis; while a particular experiment or observation may be the terminal point of an investigation which had entailed an extensive collection and analysis of data prior to that point, but which required additional data definitively to settle the questions at issue. Again, the data yielded by observation and experiment must generally be subjected to an analysis by the observer or experimenter himself in order to estimate their significance for his problems, to determine whether additional data are needed and, if so, how these may be secured.

But many scientific investigators have not the taste for such an extensive analysis of data as is requisite to the solution of certain types of scientific problems, and often to determine whether such problems are legitimately formulated in the first place. There are grounds for the belief that many problems in theoretical biology are of this sort, and that they cannot be settled without resort to a more systematic and extensive analysis of the data bearing thereon than most biologists have been disposed to favor. It is to certain problems of this sort and in this field that the present volume is addressed. The contribution which it offers is the more extensive analysis of the data relative to these problems which seems necessary to their solution. In concen-

trating on such an analysis, it is not implied that observation and experiment are of lesser importance. On the contrary, we expressly recognize that everything scientific must begin and end with the data yielded by this phase of scientific procedure. We are only attempting to supply part of the additional analysis to which these data must be subjected, in our special field, if we are to solve the problems upon which they have, hitherto unavailingly, been brought to bear.

The problems investigated pertain to the nature and operation of the primary factors in the activity and evolution of living organisms, including problems relative to psychophysical relationships, particularly as represented in human behavior and experience. This investigation entails the examination of Lamarckian and anti-Lamarckian hypotheses respecting the origin of hereditary variations, mechanistic and vitalistic theories of vital phenomena, and various philosophical conceptions of human experience. The outcome of the investigation is, on its positive side, a pluralistic theory of life and of human experience, which, whatever its merits and defects, appears to be sharply marked off from theories previously propounded.

Most valuable criticisms and suggestions relative to the problems and methods of the inquiry have been contributed by members of the Department of Philosophy of Columbia University, particularly Professors John Dewey, W. P. Montague, H. W. Schneider and F. J. E. Woodbridge. We are under particular obligations to Professor Montague for his unfailing encouragement and helpfulness since the beginning of the inquiry several years ago. We are also greatly indebted to Professor H. S. Jennings of Johns Hopkins University for the contribution of the Introduction; and for a searching detailed criticism of the original draft of what is now the chapters on Lamarckian and anti-Lamarckian hypotheses respecting the origin of variations. It may not be improper to add that Professor Jennings' willingness to associate his name with a volume which presents vigorous criticisms of his views on certain biological questions, and of which he is himself quite critical, particularly as to the methods exemplified therein, is another instance of his fine scientific spirit, a quality which is of course recognized by all who are acquainted with his work. Of others who have aided with criticisms and suggestions, mention should be made of Mr. Clarence Darrow of Chicago; Professor M. F. Guyer of the University of Wisconsin; Professor W. R. B. Robertson of the University of Missouri; and Professors E. H. Hollands, H. H. Lane, C. F. Nelson and O. O. Stoland of the University of Kansas. It is perhaps unnecessary to say that none of those mentioned are responsible for the views presented in this book, however much their thought may have contributed to the elaboration thereof.

The greater part of the material incorporated in the chapter on "Incomplete Correlations Between Vital and the Associated Physicochemical Processes" has already been published in *The Monist* for April, 1924.

SEBA ELDRIDGE.

Lawrence, Kansas,
June, 1925.

INTRODUCTION

By

H. S. Jennings

Every student of what occurs in living things meets sooner or later the question as to the fundamental nature of these processes, and of the principles to be employed in accounting for them. What is the relation of these processes, of these principles, to those dealt with in the non-living? How are biological events brought about? By perceptual conditions that can be discovered through observation and experiment, or by entelechies, agents that are non-perceptual? Or are they not brought about—determined—at all, but "free"? Must the investigator in biology, for thorough and adequate results, work on principles differing in some fundamental way from those that are adequate for the investigation of the non-living? Or will similar principles work for all science? Or is the biologist, as some contend, condemned to work on the same principles as the investigator of the inorganic, though foredoomed to imperfect and inadequate results, because the processes are determined in other ways that are not open to investigation? What, concretely, are the principles which the biologist must employ? What sort of agents are at work in living things?

If he is not to work blindly, the student must think through these questions and come to some working conclusion upon them. His task is greatly simplified if he can have before him a unified conspectus and analysis of the different answers that may be given to these questions, with the chief considerations that may be urged for and against them. The number of possible doctrines is not great and the pertinent arguments are relatively few; so that with their clear presentation many of the difficulties disappear. Such a presentation Professor Eldridge gives in the present volume. He has digested the many recent discussions on these matters; he has surveyed the chief doctrines of vitalism, mechanism and their congeners, with the arguments offered for and against them, and sets these forth clearly. He prepares for us a map of this wilderness, marking the paths leading to different destinations; and putting up at each fork a sign calling attention to the different fates in store for us, according as we take the path to the right or that to the left.

The mapmaker gives us, along with his map, his own preferred route. He indicates at each fork what seems to him the proper path, and he brings us up thus at a particular destination. In other words, Professor Eldridge presents a theory of his own, with the grounds for that theory. His conclusions lie in the opposite direction from those reached by the present writer over *his* preferred path of radically experimental analysis. For the serious student attempting to determine his own route, the map is more important than the recommendations as to preferable roads. The latter are valuable, in that they put the explorer on his mettle to decide whether he must or must not accompany the author to his conclusions. If he cannot go with him, he is forced to clear his mind by showing at just what fork in the road he will diverge, and why. The severest test for the usefulness of such a work is that it shall make clear the points where divergence might occur, with the nature of the grounds for one decision or the other. It is high praise to say that the present volume seems to qualify measurably under this test.

For certain other general problems the volume is helpful. The author includes with the rest a detailed analysis of the problem of the "inheritance of acquired characters." To those who, like the present writer, feel that profitable treatment of this matter must be mainly by immediate induction from the results of experimentation, this part will have less appeal. Yet such a systematic compendium of the pertinent considerations, *a priori* and otherwise, upon this question; and of its relations to the more general problems of mechanism and vitalism will be found useful.

The work too is a concrete example under the conviction that "science when divorced from philosophy inevitably falls a prey to faulty logic and uncritical metaphysics"; and that, in addition to observation and experiment, science has need of farreaching inferential ratiocination for its proper working out. Widespread is the opposed conviction, that the farther inference gets from experiment, the more likely it is to go wrong, so that little confidence is to be placed in conclusions reached by "extraordinary use of inferential processes of reasoning," as are the author's. Besides its use as a compendium of the matters to be weighed and considered in theoretical biology, it gives opportunity for judging the fruits of such extensive ratiocinative treatment of scientific data.

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CHAPTER I

THE ORGANISM AND THE ORIGIN OF HEREDITARY VARIATIONS

Universal Properties of Living Organisms

Perhaps the most conspicuous property of living organisms is their community with the environment. Not only are living beings continuously dependent on the oxygen, water and food substances which the environment supplies, but the chemical substances of which the body is composed are all derived from the environment. So intimate is this relationship that organisms might be, and often are, conceived to be just parts of the physicochemical world which are organized in a certain way. This conception has, quite apart from its truth or falsity, emphasized the fact that the organism is bound up with the environment at every stage of its existence, if it does not indeed stand to the environment in the relation of a part to the whole. Between them there is a constant interaction which is never interrupted until the organism perishes and becomes an unorganized part of the physicochemical world. What is true of the organism as a whole is true of its component cells and tissues. They all have, at every moment of their existence, an environment without which they can neither exist nor be conceived.

Now, although biologists often speak of a *direct* action of the environment on the organism, we may safely say that there is, strictly speaking no such process.[1] For the organism always responds to the stimulus which impinges upon it, be that stimulus what it may. It is true that if we conceive the organism as something static or inert, stimuli may be said to act directly on it. A falling stone may crush the unlucky animal upon which it descends, the rays of the sun must actually leave an impression on the growing plant or the ploughman toiling in the field, but, *as organisms,* they react to these stimuli. Only as unorganized lumps of matter are they directly modified. It

[1] Cf. Wilson, E. B., *The Cell in Development and Inheritance,* 2d ed., p. 430.

is these responses to stimuli that are of concern to the biologist,[1] and not any direct effect on the stuff of which the organism is made. The death which results from the descending stone, and the sweat which flows under the sun's impinging rays, are the phenomena which biology takes account of, as well as all the bodily processes of which these phenomena are the more conspicuous manifestations. If the crushed animal behaved as it did before, or the laborer were not at all affected by the sun's rays, we should not have vital phenomena at all, but only physical or chemical changes. Even where the stimulus affects a cell or tissue without the intervention of other cells or tissues, the action is still not direct, but there is some sort of adjustment to the stimuli. And the processes of this response are exceedingly complex, as every cytologist knows.

At every stage of the interaction between organism and environment, therefore, a complicated set of responses comes into play. And if, as must be admitted, an organism cannot exist apart from an environment, these processes of response are never interrupted. Every cell and tissue, as well as the organism as a whole, is in contact with an environment, and reacts to that environment, at every moment of its existence. These processes of interaction between the organism and the environment, including the interactions between component parts of the organism and *their* environments, constitute what is termed functional, or physiological, activity. Without such activity, neither the existence of, nor change in, the organism or any part thereof is possible or conceivable.

A third property of the organism is that of organization itself. The term connotes not only the structure of the organism statically regarded, but the variability of this structure, both in the individual and in the race. This latter characteristic might be called the property of *re*organization. The stability of hereditary structure represents, then, one side of this general property; and the tendency toward a reorganization of hereditary structure represents the other. This property might also be conceived as the organization, in matter, of the physiological processes, together with the tendency of this organization to vary. Or, to vary our terms again, it is the morphological property in vital activity, the property which has manifested itself in countless forms of life in the past, and is destined to take countless other forms in the future.

Now, the thesis we shall maintain in the present work is that these three properties—physicochemical, physiological and morphological—are present in all vital activity whatsoever, and that it is impossible

[1] Cf. Haldane, J. S., *Mechanism, Life and Personality*, p. 78.

ever to dissociate them. Living organisms (1) include parts of the *physicochemical world,* (2) which are *organized* and (3) which *function* in certain specific ways, and all these properties are necessary to the existence, and to any real conception, of living organisms. The same statement applies to the semi-autonomous parts of the organism —the cell, the tissue, the organ and the organ system. We have in any of these cases a single indivisible unity which cannot be dissociated into its properties, although any of these properties may, for purposes of analysis, be considered in abstraction from the other properties with which it is always identified.

Failure fully to grasp this truth has been responsible for much confusion in the treatment of biological problems. Environment, functional activity and morphological properties are often assumed to be independent factors, though not explicitly so, and the attempt is made to apportion credit among them for the production of evolutionary changes. Thus, Lamarck and Eimer assign the major credit for such changes to functional activity (use and disuse), although attributing much to "direct action" of the environment; Naegeli, Bergson and other vitalists give the principle rôle to morphological properties (a "vital impetus," "internal evolutionary force," etc.); while Loeb and other biological materialists attempt to interpret all vital phenomena in terms of physicochemical substances derived from the environment. We may say, incidentally, that the Lamarckians have on the whole shown more restraint in their theoretical discussions than have biologists of the vitalistic and materialistic schools, since none of them, so far as we know, have attempted to explain modifications of hereditary structure by use and disuse alone, whereas extreme representatives of the other schools—for example, Loeb and Naegeli—have sought to explain modifications exclusively by the factors (or properties) which they are severally concerned to champion.

What they have all failed to see, apparently at least, is that all three factors or properties are indissolubly associated together, and that an organism can neither exist nor be conceived if any of them be wanting. We cannot make the experiment of isolating one or two of the so-called factors from the remaining factor or factors and determining how the single factor or two-factor combination will behave; nor can we ever find the experiment ready made in nature. If we leave out structure and functional activity we have remaining nothing but physicochemical substances; if we take away physicochemical substances, structure or functional activity, or, again, all but function or structure, we have nothing left to which anything in the world of life corresponds.

The physicochemical interpretation cannot, of course, be disposed

of in so summary a fashion, as this hypothesis purports to account for structure and function themselves in physicochemical terms. It is incumbent on us, therefore, to justify the criticism here directed against that theory; and this we shall undertake to do in the sequel.

Let us consider more in detail the way in which these three properties are associated together.

The environment is made all-powerful by those biologists who attempt an interpretation of vital activity in physicochemical terms. All the matter incorporated in living organisms is ultimately derived from the inorganic world, and if we go far enough back in time, we must, according to the hypothesis logically interpreted, transcend the organic altogether and get over into the inorganic. Life must, therefore, have arisen from some unknown combination of chemical elements, or, rather, it was that combination itself. Organic structure and functional activity are not allowed to be ultimately valid categories at all, but only as offering problems to be solved in terms of that which is valid in an ultimate sense.

Now, this hypothesis has, quite apart from its validity or invalidity, emphasized the important truth that living organisms are in one sense part and parcel of the physicochemical world, and therefore solidary with it. We believe that vital phenomena cannot be completely accounted for in physicochemical terms, but neither can they be explained without taking their physicochemical properties into account. It cannot be supposed that matter abrogates its ordinary properties when incorporated in a living body, even though it may be organized in a way to which there is no parallel in the inorganic world. Chemical substances will continue to combine in their characteristic fashion, albeit the complexity of the organization to which they are subjected often masks the simplicity of their normal reactions. Nor do mechanical laws suspend their operation in the case of living organisms. The conservation of energy, the laws of motion, the force of gravitation,[1] the properties of electricity, the behavior of gases, liquids and solids are exhibited in the organic as well as the inorganic. Let anyone attempt to imagine what an organism would be like could there be substituted other elements for the carbon, oxygen and hydrogen which bulk so large in its composition, or some other chemical compound substituted for water with its peculiar solvent and thermal properties, and it will be seen how impossible it is to conceive an organism apart from its specific physicochemical constitution. Consider, again, the

[1] Such terms as force, matter, influence, explanation and the like are used for the present in a descriptive, non-technical sense. Their proper meanings will be considered in later chapters.

influence of gravitation on the relation of structural parts to each other. The bony skeleton of the vertebrate, for example, or the connective tissue—indeed, every structural arrangement—is adjusted to this force, and we cannot possibly understand these structures without taking it into account.

Such being the community between living organisms and the physicochemical environment, the question of what hereditary modifications can be produced by the environment, if taken in a general sense, is a meaningless one. The organism and the physicochemical world are in part identical, and so intimate is the union between them in all the manifestations of vital activity that there can be no question of what effect the environment at large may have on the hereditary characters of the organism. We can, of course, vary environmental conditions and observe the physiological and morphological changes that may be produced, but this is obviously substituting one environment, or part of one environment, for another. Such substitutions certainly offer a legitimate field of inquiry, but the problem of whether hereditary modifications are ever determined by environmental influences alone or, instead, by functional activity or germinal variations alone is an artificial one, since all three processes are invariably present whenever these modifications are produced, and it is impossible to apportion credit among them for the changes produced. From one point of view the living organism seems to be just a part of the physicochemical world which behaves in certain ways; from another point of view the physiological processes whereby the organism is related with the environment appear to be the most significant feature of vital activity; from still another point of view it is the hereditary constitution of the organism which plays the dominating rôle, representing, as it does, the physicochemical composition of the organism and the physiological processes which relate the organism with its environment. All these views are correct, but they are correct only as *views*, and not as expressing the complete reality of which they are the views.

Physiological processes are as primary and fundamental as are environmental conditions, and perhaps offer a better clue to the understanding of vital activity than do environmental conditions or hereditary structure. These processes not only relate the organism with the environment, but the organism itself could be conceived as the complex of physicochemical forces built up by their agency. Environmental stimuli never act directly, as we have insisted, but the organism assimilates them to its uses in its own characteristic fashion. These processes can be interpreted physicochemically, after a fashion, but

the physicochemical school have not succeeded in explaining, on their hypothesis, the more characteristic features of functional activity.[1] We may therefore conceive it as something *sui generis,* although, as we must never forget, there are always physicochemical *conditions* of successful, as of unsuccessful, functional activity. In either case the physicochemical agencies involved continue to obey physical and chemical laws, but it is the failure or success of the organism's attempt at a higher synthesis of these agencies which means death or diminished prosperity in the one case, continued existence and a balance of prosperity in the other.

"Natural selection" is more fruitfully conceived thus than by vaguely personifying it as a selective agency residing in a general environment. The succession of stimuli (food, temperature, predaceous attacks, etc.) and of responses thereto, which together constitute failure or success, may not be precisely the same for any two members of a species which have struggled to maintain themselves. What we want is specific explanations of success and failure, and not the merely formal explanation which "natural selection" or "survival of the fittest" offers us. The processes of selection are as numerous and as varied as are the conditions affecting the living being, or, rather, as varied as the combinations of stimuli and of the sequent responses in time, with the varying individual careers in which they are factors. Obviously the process is not a single or a simple one, but as varied as are the life histories of individual organisms. The eliminating effects of selection may be due primarily to hereditary defects of the organism, or they may be due to a chance failure of the conditions to which organisms of the type in question have become adapted; or elimination may be due to both types of factors combined. But the effects of either or both sorts of factors will be seen in a derangement of physiological activity. The eliminating conditions impose an activity on the organism for which it is not fitted and which may result in its speedy death.[2]

Here, as before, it is impossible to dissociate the three properties of the general process and decide which is responsible for the final result. If the environment be defined as the *external* objects which condition the organism, such a determination would often be pos-

[1] Cf. Haldane, J. S., *Mechanism, Life and Personality,* Lecture II; and our own discussion of physicochemical hypotheses, Chaps. VII–XII.

[2] The biometrical school of research has been conducting quantitative investigations of natural selection and other general processes involved in phylogenesis and attempting exact determinations of the influences exerted by the various factors therein. Karl Pearson's *Grammar of Science,* second ed., Chaps. X and XI, indicates some of the possibilities in this direction.

sible; but no such restriction of the term can be accepted. Environment in the wider and more valid sense has implanted itself in the very constitution of the organism, and it is often because environment in this sense is too insistently present, and too unyeilding in its demands, that the organism is unable to maintain its existence. The most we can say is that failure of the environment to repeat itself in certain particulars, or failure of the organism to avail itself of environmental repetition in those particulars, is what results in the organism's elimination. And if the cause of failure (or of success) was some hereditary weakness (or strength) of the organism, that cause was itself, in part, a product or result of physiological activity, since whatever change is produced in any organism must come by way of physiological processes.

Indeed, the renewal, the very existence, of structure is unthinkable apart from physiological activity, since it is by means of this activity that the materials in which structure is embodied are brought together and molded into a coherent form. The obvious dependence of structural vitality on normal metabolic processes sufficiently emphasizes this truth. The materials which embody structure are in constant flux, although the order or form imposed on these materials is highly stable. We may even go so far as to say that structure is only a statical view of a great and complex system of physiological processes, the aspect of order or form in a ceaseless process of change. What we are concerned to emphasize is that an organism is inconceivable apart from physiological processes, these processes being as indispensable to the existence and to the idea of organisms, as we know them, as are the materials constantly interchanged with the environment by their agency.

It is, then, not possible that hereditary modifications of structure should ever be produced without functional activity being involved in the process, or that, on the other hand, such changes should be produced by functional activity alone. The material substances involved will always play their part, imposing limits upon, as well as making positive contributions to, the higher synthetizing activity which we call life. We can within limits vary this physiological activity and observe the morphological changes that may be induced, but it is never possible to abrogate it altogether. Such experimental variation of physiological activity constitutes a legitimate field of inquiry parallel to that offered by varying environmental conditions, but there can be no real inquiry as to the hereditary modifications produced by physiological activity in general. Such activity is present whereever life is present, and we might as well seek to determine what

rôle life in its most general sense plays in the evolution of its own specific forms, as to attempt a determination of the rôle played by physiological activity. That is, relational processes within the organism and between the organism and its environment constitute a fundamental property of life, which is necessary to the existence and to the conception of life.

"Use and disuse of parts" is therefore a relative term, since no living part of an organism can fall into a complete disuse. If a part ceases to function in some sense it ceases to exist, except perhaps as extraneous matter which might without risk be cast off altogether. We may nevertheless recognize a comparative disuse of parts, or organs, a disuse as compared with a greater use of the same parts at other times or in other organisms, but the greatest caution is necessary in drawing inferences from such a disuse, because of the fact that functional activity is an indivisible whole which affects, as well as being affected by, the activity of each special part.

Finally, add to these considerations the fact that causal relationships within the organism are still very largely undetermined, together with the fact that organic chains of cause and effect are indissolubly associated, when not identical, with physiological processes, and we shall have the strongest grounds for caution in denying any sort of credit to physiological activity for the production of hereditary modifications.

Just as living organisms cannot exist or be conceived apart from functional activity and an environment, so they cannot exist or be conceived apart from a property of organization. The very term organism connotes this property, as does the division of nature into inorganic substances or unorganized bodies, on the one hand, and living beings or organized bodies, on the other hand.

Organization may be defined in the most general sense as the coordination of specific physiological processes in and through a material body of a certain shape, size and physicochemical composition. Organization in this sense is obviously present wherever life is present, and is as necessary to the existence of life as are physicochemical substances and physiological processes.

A fundamental characteristic of vital organization is its very high degree of stability. This characteristic is, of course, much more pronounced when living beings are regarded from the viewpoint of phylogeny, than when considered from the standpoint of ontogeny. The individuals of most species traverse several stages of morphological development, while the individuals of one generation repeat, with

relatively slight variations, the stages of development traversed by their forbears of the preceding generation. But these variations have made the evolution of species possible, and variability must therefore be put down as a fundamental characteristic of vital organization. Variations in organization, however, are correlated with variations—qualitative, quantitative or both qualitative and quantitative—of the associated properties—functional activity and physicochemical constitution, since, so far as living organisms are concerned, neither of these general properties, with changes therein, can be independent of the associated properties and *their* changes.

The general property of organization (as thus correlated) may, of course, be split up (analytically) into the sub-properties of hereditary organization, regarded as the measure of identity or likeness between successive generations of the given species, race or line, and variability of this organization, or that property of life which connotes its characteristic departures from ancestral forms. The latter property has been variously termed the "vital impetus," an "internal evolutionary force," etc., or simply the tendency toward variation. Whatever term we adopt, it must be taken to connote the property of variability itself, and perhaps also a tendency toward variation in more or less definite directions.

This property must have been present from the beginning, since structural changes are modifications of preëxisting structures, and these structures represented, in their turn, modifications of structures existing before them; and this process when traced back takes us to the beginning of life, or at least to the beginning of the evolution of life. If this chain of evolving structures be broken, the series is terminated, and there is no way of reinstating it.

For the present we may regard this general property of organization as in the nature of a working concept or descriptive formula, although representing, as we shall attempt to show, a category of factors in vital activity which are fundamental. It is a concept of the infinite number of forms which living beings have assumed, and which they may hereafter assume. Environment and functional activity have certainly been involved in the evolution of all such forms, but they cannot, when strictly regarded, supply a complete explanation thereof. We must assume, in addition, some sort of organizing factor, property or relation, if organization and its variability are to be adequately accounted for. This assumption it will be our business later to justify.

The general morphological factor here assumed is everywhere present, like functional activity and the environment. As we have in-

sisted before, it is not an independent factor, but only a special aspect of an indivisible vital process; and that process can neither be interpreted by this factor alone, nor yet interpreted without its aid. The morphological, the functional and the environmental are indissolubly joined together, and are jointly responsible for all the achievements and all the failures of the organism. To attempt to apportion credit among these three factors for the successes or failures of the organism rests on a failure to realize this truth. It is in most cases as fantastic an enterprise as would be the attempt to assign to some aspect of a material object—say the perspective of a tree—a causative efficacy in determining changes in that object. The most we can do is to affirm that changes cannot be understood without taking all those factors into account.

What has been said regarding the morphological factor applies, of course, to the germ-plasm, or germ cells, which carry the structure and its tendencies from generation to generation. In any hereditary modification of an organism, germinal organization, functional activity and physicochemical agencies invariably coöperate together. Any acquired characteristic, so-called, is an expression—it may be, the fullest possible expression—of a germinal potentiality, but a potentiality, we must suppose, which would have remained latent if environmental stimuli and functional activity had not called it into play. And considering the fact that functional activity is a unified whole, and that every specialization of activity contributes to the whole, and in some sense to the maintenance of each special part, we are not justified in saying in advance of an experimental determination just what is the interchange of services between the several parts.

It is, for example, rash to say that the body and its processes merely lodge and feed the germ-plasm, and that in exchange for this service the germ-plasm supplies to the body the many variations which determine its admirably coördinated structure, and its highly serviceable functions. In so doing it is assumed that the body and the germ-plasm have yielded us the secrets which warrant such a sweeping generalization, an assumption which is quite premature. E. B. Wilson says, in dealing with one side of this relationship: "What lies beyond our reach at present . . . is to explain the orderly rhythm of development—the coördinating power that guides development to its predestined end. We are logically compelled to refer this power to the inherent organization of the germ, but we neither know nor can we even conceive what that organization is . . . Despite all our theories we no more know how the organization of the germ cell involves the properties of the adult body than we know how the properties of hydrogen

and oxygen involve those of water." [1] The process on the other side must be equally complex, for it is the reverse of the process of individual development, but with the important difference that this process results in the organization within the minute germ cells—almost infinitesimally small compared with the body—of the carriers of every bodily character, and in so perfect a manner that the germ rarely goes astray when its turn for development comes.

These facts should make us wary of asserting so confidently that variations in the germ-plasm are determined by processes starting in the germ-plasm itself. If, as Wilson says, an explanation of development is beyond our reach, though aided in our researches by high-power microscopes, how much further off is an explanation of the reverse process of involution—the involution of somatic characters within the germ, when the microscope whereby we examine it is, so to speak, reversed. The truth is, our microscopes do not serve us at all in the examination of this reverse process, and we are compelled to rely on speculation, or, at best, on an indirect inquiry into what the process may be. The case would be different were the contributions of the soma to the germ so simple as Weismann makes them out to be. There would then be no reverse process of involution to be explained, but merely the unfolding of characters preëxistent in the germ, or a process which is virtually equivalent thereto. But such a conception does violence to the facts of vital activity, as we have attempted to show.

[1] *The Cell in Development and Inheritance,* second ed., pp. 432, 433. The second edition of Professor Wilson's work, from which the quotation in the text is taken, was published in 1900, and does not accurately represent his present conception of the developmental process. He now favors an explicitly mechanistic interpretation of the process, though admitting that the current interpretations are inadequate. In the third edition of the same work (1925), pp. 1115, 1117–1118, he writes: "It is our scientific habit of thought to regard the operation of any specific system as determined primarily by its specific physicochemical composition. We continually refer the particular mode of development of an organism to its so-called 'organization' . . . Concerning the fundamental nature of this organization we are still ignorant; but we have nothing to gain by the vitalistic assumption that the guiding principle in development is not only unknown but unknowable. Existing mechanistic interpretations of vital phenomena, evidently, are inadequate; but it is equally clear, as someone has said, that they are a 'necessary fiction'. Knowledge will be advanced most surely by assuming that the problems of the cell can be solved by converging upon them all our forces of observation and experiment. If we are confronted still with a formidable array of problems not yet solved, we may take courage from the certainty that we shall solve a great number of them in the future, as so many have been in the past. If Mendelian heredity, at first sight so inscrutable, is effected by so simple a mechanism, we may hope to find equally simple explanations for many other puzzles of the cell that lie beyond our present ken." Quotations in text and footnote authorized by The Macmillan Company, publishers of the work cited. The second and third editions were copyrighted in 1900 and 1925 respectively.

Before leaving this part of the discussion, we must record our recognition of the fact that the morphological factor, like the functional or environmental factor, may, within limits, be varied and the results in individual and racial development observed. Various experiments on the germ cell and on the early stages of the embryo can be performed, which will produce divers abnormal forms, and in some cases individuals capable of maintaining themselves and of leaving offspring having similar modifications. These facts are significant, but here, as in the case of function and environment, there can be no question of experimenting on one property without at the same time modifying the associated properties. The germ cell carries not only the determinants of bodily structures, but also the determinants of the functions and of the physicochemical properties with which structures are bound up. The most that can be done in experimenting on a given property is to take cases where it is more pronounced than are the associated properties, and, so, where a balance of experimental variation may be deemed to be on its side. Such experiments are legitimate when conducted with a knowledge of this limitation, and may have valuable results, but their significance cannot, because of this limitation, be so farreaching as many biologists are disposed to assume.

Summing up the foregoing discussion, we may say (1) that living organisms are in community with their environment, being embodied in physicochemical substances drawn from the environment, and existing by and through their interactions with the environment; (2) that there is a continuous activity within, and interaction among, the component cells and tissues of the organism, and that these relational processes within the organism, as well as the processes directly relating the organism with the environment, are necessary to the existence and to any real idea of the organism; (3) that the organism always has a structure which is highly stable, but which is nevertheless more or less variable, as regards both the individual and the race. These fundamental properties of life being always present, it is impossible to single out any one of them as exclusively determining hereditary modifications of the organism; we are obliged to say, on the contrary, that each property is involved in every such modification, and must be taken into account if any given modification is to be understood. That is, we must take these several properties as features of a single indivisible process, and therefore as having a representative, rather than an existential, value. Experimentation for the purpose of determining the rôles played by these properties in the genesis of hereditary modifications is therefore judged to be of questionable validity, although it is conceded that results of a certain value may be attained

by selecting for investigation cases in which a given property is more emphasized than are the associated properties.

Although any student of biology would concede the correctness of the foregoing analysis, when regarded as a descriptive account, in general terms, of the organism and its activity, but seldom in the history of biological thought have the full implications of such an analysis been grasped and held firmly in mind. Since structure, functional activity and physicochemical agencies are always present in vital processes, it is possible to claim that any one or two of these general properties, or even selections therefrom, play the determining rôle in the genesis of variations; and the claim can be made to appear plausible according as one stresses the property or properties chosen for purposes of a causal explanation, and fails to recognize the essential identity of all three properties in a single process. The history of the theory of evolution is replete with attempts of this nature. If the analysis thus far be well grounded, such attempts must be adjudged as of very doubtful validity.

To this *a priori* demonstration may be added one of a more positive character. If the three general properties in question have all been involved in the evolution of living organisms, it should be possible to cite characters which show unmistakably the impress of any given property. That is the task to which we now turn.

The procedure in this more positive part of our analysis will be to examine critically the current hypotheses respecting the origin of variations, which deny or minimize the rôle played therein by one or two of the general properties recognized, while magnifying the rôle played by the other property or properties. In our examination of these hypotheses the aim will be not only to expose their more extreme claims, based on this discrimination between equally essential properties, but also to identify and appraise their positive contributions to the theory of variations. It should be possible to arrive in this way at a fairly synthetic theory respecting the origin of variations, even though it be of a provisional nature only.

Current Hypotheses Respecting the Origin of Variations

One group of hypotheses deny, minimize or distort the rôle played by functional activity in the genesis of variations. Ever since Weismann demolished much if not all the evidence adduced before his time in support or illustration of the Lamarckian hypothesis, biologists have framed theories of the origin of variations based on the assumption that the operation of the so-called Lamarckian factor in phylogenesis

had been definitely disproved. A majority of biologists at the present time accept one or another of the theories so based. We could designate all these theories as anti-Lamarckian in intent, although they have their positive features as well.

Three types of anti-Lamarckian theories may be distinguished. (1) The most important of these types holds that hereditary variations have their origin in the germ cells, without the soma or body cells playing other than an instrumental rôle therein. According to this type of theory, the specialized functional activities of the body have no influence, or at least no representative influence, in the determination of hereditary variations. Included under this general group of theories are (a) the germ-plasm theory propounded by Weismann, (b) the mutation theory in the general form given it by De Vries and accepted by most biologists at the present time; and (c) the genotype conception of the organism more recently developed by Johannsen and others on the basis of pure-line experiments, and erected into a general theory of heredity applying to all types of organisms. These three sub-types of the germinal theory are of course not mutually exclusive, but are to be regarded, rather, as filiated historically, in the order named,[1] the later sub-types modifying and elaborating the earlier type or types on the basis of new data. The adherents of this general type of theory are not fully agreed among themselves respecting the originating causes of variation in the germ cells, although they would all, except the vitalists, ultimately refer such changes to chemical and physical agencies. Their differences regarding this question will be duly considered in the proper place.

(2) A second type of anti-Lamarckian theory attributes the origin or hereditary variations to "direct action" of the environment on the germ cells. The action of the environment may, according to this theory, affect the germ cells alone, producing modifications in the offspring which are or are not hereditary; or it may affect soma and germ cells simultaneously, producing modifications in both, though only the direct modification of the germ cells can be inherited. The first type of action is demonstrated by a mass of experimental data and is no longer questioned by any one; the latter type of action, often discussed under the rubric of parallel induction, is more problematical, at least where the germinal modifications produce the same sort of somatic

[1] Regarding the relation of the mutation theory to the germ-plasm theory see Conklin, E. G., *Science,* N. S., Vol. XXI, 1905, p. 525; for the relation of the genotype conception to the germ-plasm theory, see Johannsen, W., "The Genotype Conception of Heredity," *Am. Nat.,* Vol, XLV, 1911, p. 132, and East, E. M., "The Genotype Hypothesis and Hybridization," *Am. Nat.,* Vol. XLV, p. 162.

modifications in the offspring as were produced in the parents by the original stimulus. Evidence from immunity, alcoholic poisoning and pathology demonstrates, however, that certain classes of stimuli affecting the parent may similarly affect the offspring, whether or no this may be interpreted as a phenomenon of heredity, in the strict sense of the term.

We shall concern ourselves chiefly with that part of the theory of direct environmental action which is referred to as parallel induction, since in nature "direct action" having a phylogenetic value would usually, though not necessarily always, affect germ and soma simultaneously. The parallel induction theory is not opposed to the first type of theory, but is held, indeed, by all biologists who reject the Lamarckian hypothesis,[1] since every one agrees that herditary modifications may be induced, in various species of organisms, by a large variety of environmental stimuli. All the sub-types of the germinal theory would agree that a considerable share of hereditary variations in the germ cells are induced by such stimuli. Their disagreement on the general question would concern the originating causes of changes not attributable to external stimuli.

This hypothesis may be subsumed under the Lamarckian theory, after the manner of Plate, by holding that all acquired characteristics, whether of germ or soma, must, when hereditary, be put down to the credit of the Lamarckian principle.[2] This interpretation of the Lamarckian hypothesis does not contradict the theory of Lamarck himself, since the latter allowed for direct action by the environment in phylogenesis, though restricting this type of action to the plant kingdom. But the doctrine of direct action should be kept distinct from the hypothesis which attributes a certain type of hereditary variations to specific or representative effects of functional activity. The latter may, following traditional usage, be designated as the Lamarckian hypothesis.

We may observe, finally, that physiological activity of some sort is involved in changes subsumed under the rubric of parallel induction, since there is really no such process as direct action of the environment on the organism. The real question, therefore, is whether this and other hypotheses denying any representative effects of functional activity on hereditary characteristics can account for all classes of hereditary changes.

(3) A third possibility, sometimes suggested as a more or less

[1] The parallel induction theory may also be combined, in an eclectic fashion, with a modified type of neo-Lamarckism, as by Ludwig Plate.

[2] *Selectionsprinzip und Probleme der Artbildung,* 1908 ed., p. 336.

definite hypothesis,[1] is that functional activity as such may have an effect on hereditary characters, but not a specific or representative effect, except by chance. This hypothesis corresponds to Thomson's second and third degrees of modification transmissibility.[2] His second degree of modification transmissibility refers to modifications in the offspring derived from acquired modifications of the parent and affecting the same tissues, though in a different fashion. Such hereditary modification might be illustrated by the fact that certain acquired diseases apparently determine a predisposition thereto in the offspring, but not the diseases themselves. His third degree refers to transmissible modifications, but modifications unlike those acquired by the parent, and affecting different regions of the body, as if, to cite Thomson's own illustration, the sons of fathers who had eaten sour grapes should have wry necks.

While this hypothesis is not compatible with the germinal theory of the origin of variations in its strict form, any one is at liberty to combine the two, if he sees fit, by making such modifications in the former as may be deemed necessary.

The hypotheses we have sketched, together with the neo-Lamarckian hypothesis itself, seem to exhaust the logical possibilities of the rôle played in phylogenesis by functional activity. That rôle is conceived to be purely instrumental—one might almost say transportational—as by the germinalists and direct actionists; specific or representative, as by the neo-Lamarckians, strictly so-called; or causally effective, but non-representative in its results, as in the third type of hypothesis. We propose to test these various theories by a large class of facts with which none of them, and especially the anti-Lamarckian theories, have hitherto been squarely confronted. We refer to certain types of active adaptations to the environment. We shall also test the possible combinations of the anti-Lamarckian theories by the same group of facts.

Some forms of the neo-Lamarckian theory have, in their turn, ignored, minimized or misinterpreted the rôle played in phylogenesis by factors coming under the physicochemical or morphological categories. The type of theory advocated by Pauly[3] and other psycho-Lamarckians could be thus characterized, as could also, perhaps, the earlier types of neo-Lamarckism represented by Spencer and Eimer. We propose to subject such versions of the Lamarckian principle to

[1] Cf. Thomson, J. A., *Heredity*, 2d. ed., pp. 191 ff., 223.
[2] *Op. cit.*, pp. 191–193. Thomson does not argue for this hypothesis, but merely suggests its possible application to some hereditary changes.
[3] *Darwinismus and Lamarckismus.*

positive tests also, tests supplied by evidence ignored, minimized or misinterpreted by these hypotheses.

Another type of theory denies any ultimate validity to the functional and morphological categories, and purports to explain all functional and morphological characters in physicochemical terms. In pursuance of our program, we shall subject this type of theory to certain positive tests in order to determine whether it can account for the phenomena of vital activity, including the genesis of hereditary variations. Physicochemical theories are of course directed, on their controversial side, against vitalistic theories of the organism and its evolution. They are not necessarily anti-Lamarckian in intent, but rather are indifferent to the problem set by the Lamarckian hypothesis. Functional activity and any rôle it may play in phylogeny, whether representative or non-representative in character, are regarded as of subordinate interest, and without genuine scientific significance.

Just as the physicochemical hypothesis is preoccupied on its controversial side with vitalistic theories, and attempts to demonstrate the unsoundness of those theories, so the vitalistic theories take the converse position, and attempt to demonstrate the inadequacy of the physicochemical conception of life. Just as we have one set of antagonistic theories in the germinal and the Lamarckian theories, each being framed and its claims urged with reference to the other; so we have a somewhat independent set of antagonistic theories in the physicochemical and vitalistic theories, each attempting to refute the other. The vitalistic theories may or may not be opposed to the Lamarckian theory, depending on the attitude taken by the particular theory toward the controversy between Lamarckians and anti-Lamarckians respecting the origin of variations. The types of vitalism represented by Driesch and Bergson deny or minimize the rôle of functional activity in phylogenesis, while all types of psychovitalism, or psycholamarckism, as it is more often termed, concede fully the claims made for the Lamarckian factor, while tending to ignore or discount the rôle played by factors coming under other general categories. Naturally, all types of vitalism, because of their controversial position, tend to minimize the rôle played by physical and chemical agencies in vital phenomena. The tests to which the sub-types of the vitalistic theory must be subjected will, therefore, depend on the theories themselves.

If justified in our contention that physicochemical processes, functional activity and morphogenetic factors must all play their part in every vital process, including phyletic changes, and that they must all leave their stamp on the hereditary characters of the organism, then it should be possible to adduce facts that will confute any hypothesis

which denies the causal significance of any given factor or property. The evidence refuting any theory of a partial nature will therefore depend on what such a theory denies.

Special Problems Raised by the Inquiry

Before going on to consider these theories it may be well to anticipate some special problems which are likely to emerge from the discussion. The three general properties of organisms which have served as the starting point of the analysis are of course open to the most casual observation. They are characteristic of all living things, and must be regarded as essential to the existence and conception of life, as we are acquainted with it. This fact, commonplace though it be, carries with it a destructive criticism of all theories respecting the origin of variations, which select one or two of these general properties, or specific properties included therein, for purposes of a causal explanation. This we shall attempt to demonstrate at length.

As the discussion proceeds it will be convenient, and even necessary, to split up these general properties into more specific properties, in order to apply more crucial tests to the theories under examination. The environment, for example, covers a wide range of chemicals, energies and living organisms, including fellow-members of any given species that may be the subject of the analysis, as well as members of other species entering into the vital economy of which the given species is a factor. The environment also includes, for some species at least, traditions and other cultural factors originating in and through the social life of the given species in the past. This fact is recognized in the distinction made by sociology between the physical, geographic or "natural" environment, on the one hand, and the cultural environment, on the other. These phases of the environment are of course not separable from one another, but the environmental adjustments of living organisms must be analyzed in terms of factors of both types, where cultural factors are operative. Functional activity likewise covers a wide range of particulars, including all the physiological processes of the body, instinctive responses to external stimuli, intelligent behavior and so on. The morphological property covers an indefinitely large number of items also.

The consideration of the interactions among the various factors in vital phenomena is likely to raise certain metaphysical questions of more or less crucial significance for the special problems under examination. The materialistic conception of life itself raises the question whether there are factors operative in life which cannot be subsumed

under the general concepts of matter and energy. The matter and energy combined in the organism are ultimately derived from the inorganic world, and if the organism is something more than this matter and energy, what is the nature of that something more? That is a question which cannot be evaded.

Again, if there are non-material factors operative in life, the question arises whether they are all of the same sort. If, for example, non-material factors are operative in intellectual activity and in physiological processes not correlated with such activity, are the factors operative in the former similar in kind to those involved in the latter?

Also, if non-material factors are operative in vital phenomena, what is the relation of functional activity to the material and non-material factors, respectively? That question, too, we must take into consideration. More particularly, if functional activity is causally involved in the origin of variations, is it to be identified, in this process, with physicochemical factors, with non-physicochemical factors, or, instead, with factors coming under both these categories?

Similar questions lurk in our distinctions between different phases of the environment. If organisms are something more than configurations of matter and energy, then the environment is not equivalent to the physicochemical conditions surrounding the organism, since part of the environment of any organism is constituted by other organisms, both of its own and of other species. Again, if traditions and other elements of culture are factors in the life of some animal species, can these be identified either with material phases of the environment, or with non-material factors such as may be operative in the organism itself?

Matter and energy themselves have been conceived in the most diverse ways, and we shall have to take these divergent conceptions into account, when dealing with materialistic theories of life. Should we find none of the current conceptions quite tenable, as seems not unlikely, what factors in nature shall those terms be taken to signify, and what types of relations subsist between these and other categories of factors in the organic?

Finally, what status, if any, shall be assigned to logical and mathematical entities, in a comprehensive synthesis of the factors and relationships involved in vital activity? What shall space and time signify in such a synthesis? And are there still other "existents" or "subsistents" which must find a place in this synthesis?

Such questions, while they may seem somewhat remote from the main problems of our inquiry, are in reality quite fundamental thereto.

The problem of the origin of variations, for example, is based on assumptions respecting certain of these questions, and this problem cannot be adequately investigated until those asumptions are examined and their implications pointed out. Vitalistic and materialistic theories of life are based on similar assumptions, and these theories cannot be correctly appraised without a critical examination of those assumptions. Similar considerations apply to current conceptions of matter and energy, and to other controversial questions bearing upon the specific problems of our inquiry.

An examination of these various questions in relation to the main problems of our inquiry and the empirical data bearing thereon, together with the positive synthesis erected on the basis of this analysis, will signify an attempt at a reformulation of these problems, a revision of the methods employed in dealing with them, a revaluation of evidence relevant thereto, and an approach along these lines toward definitive solutions of the problems thus treated.

CHAPTER II

LAMARCKIAN AND ANTI-LAMARCKIAN HYPOTHESES AS TO THE ORIGIN OF VARIATIONS: THE EMPIRICAL EVIDENCE

SINCE the anti-Lamarckians generally combine some form of the germinal hypothesis with the direct action or parallel induction hypothesis, and since, further, the hypothesis of non-representative modification transmissibility may be combined with the first two hypotheses into a composite anti-Lamarckian hypothesis, we shall consider these several hypotheses concurrently with each other, subjecting them, individually and collectively, to the tests supplied by certain classes of active adaptations. The various types of vitalism present nothing new in principle, so far as the present problem is concerned, and we shall therefore postpone our consideration of those theories to later chapters. The examination of those theories may be taken, however, as elaborating in certain respects the discussion of Lamarckian and anti-Lamarckian theories in this and the following chapters. As aforesaid, the physicochemical theories, as such, are neutral toward this problem.

To avoid misunderstanding, it should be emphasized anew that we do not propose to defend the Lamarckian hypothesis in an exclusive form. That would violate the basic assumptions upon which the analysis proceeds. We are working toward a synthetic theory of phylogenesis, as well as of vital activity in general, and therefore toward a demonstration that all partial theories on these problems must be abandoned or modified. Stated in other words, we contend that it is not possible in fact, or in principle, to apportion credit for phyletic changes among *equally essential factors* or properties of vital processes. However, it is possible to demonstrate the conjoint operation in phylogenesis of all these essential properties or factors in the vital process, by considering phylogenesis from the viewpoint of the several factors taken separately, and thereby refute all theories denying the causal efficacy of any factor or two-factor combination.

Before subjecting the anti-Lamarckian theories to our positive tests, it will be well to define the present logical position of these theories,

and survey briefly some of the more significant evidence bearing thereon. We shall examine first those features of the germ-plasm theory, as developed by Weismann and his followers, which are of special significance for the questions under consideration.

The Germinal Hypothesis

This theory was based on two bodies of evidence which seemed to support each other. One of these concerned the inheritance of mutilations. After the transmission of mutilations was shown to be of very doubtful occurrence, the revision of current theories allowing for the transmission of these and other somatic modifications underwent a drastic revision. Galton,[1] Pflüegr,[2] and Weismann [3] himself were the most destructive critics of the evidence adduced in support of the view that mutilations may be inherited. Galton and Weismann proceeded to formulate theories of heredity which limited or excluded altogether the transmission of parental modifications to offspring. Galton himself did not deny all phylogenetic significance to acquired modifications, but conceded the possibility of their being faintly heritable.[4]

The other body of evidence adduced in support of the germ-plasm theory was supplied by observation of the early differentiation and segregation of the sex cells. Jäger [5] and Nussbaum [6] were early precursors of the theory, who based their speculations on evidence of a similar character. Weismann built a firmer empirical foundation for the theory by investigating the origin of sex cells in Hydromedusæ.[7] The latter succeeded, he thought, in demonstrating conclusively the continuity of the germ-plasm in all sexually propagating species, and thereupon proceeded to argue *deductively* that *no* somatic modifications could be transmitted to offspring, in such species, thus going far beyond the conclusions warranted by his critical examin-

[1] "A Theory of Heredity," *Cont. Rev.*, Vol. XXVII, 1875, pp. 80–95.

[2] "Ueber den Einfluss der Schwerkraft auf die Thielung der Zellen und auf die Entwicklung des Embryo," *Archiv. f. Physiol.*, Bd. XXXII, 1883, p. 68. Cited by Weismann, *Essays Upon Heredity and Kindred Biological Problems*, Vol. I, pp. 70, 81.

[3] "On Heredity" (1883); "On the Supposed Botanical Proofs of the Transmission of Acquired Characters" (1888); "The Supposed Transmission of Mutilations" (1888): *Essays*, etc., Vol. I, pp. 67–106, 397–430, 431–461; and in many later writings.

[4] *Op. cit.*, p. 94.

[5] *Lehrbuch der allgemeinen Zoologie,* II. Abtheilung, 1878.

[6] "Die Differenzirung des Geschlechts im Thierreich," *Archiv. f. mikr. Anatomie*, Bd. XVIII, 1880.

[7] *Die Entstehung der Sexualzellen bei den Hydromedusen,* 1883.

ation of evidence indicating the transmission of mutilations and other somatic modifications.[1]

The theory that the germ-plasm is continuous and not formed anew in each generation carried with it, as a simple corollary, the doctrine that no mechanism for the transmission of acquired modifications is discoverable or even conceivable. Both the theory and the deductions based thereon were eventually accepted by the great majority of biologists interested in the general problem, and the modification of hereditary characters through functional activity came to be regarded as impossible.

What is the present status of the germ-plasm theory, and what rôle does it play in current discussions of phylogenesis? We may say at once that the doctrine of the continuity and isolation of the germ-plasm in the Weismannian sense has been abandoned, *but that the inferences based on these supposed characters of the germ-plasm are nevertheless accepted* by a large proportion of present-day biologists. We may say, further, that evidence has accumulated since the theory was first elaborated which could not be interpreted on the theory in this earlier form, and which led to the formulation of several subsidiary theories to account therefor, on an anti-Lamarckian basis. The more important of the evidence referred to demonstrates, or appears to demonstrate, (1) determinate variation and (2) the initiation of hereditary variations through environmental changes. To account for these two groups of facts, the hypotheses of germinal selection and of "induced germinal selection" were proposed.

The point of interest in the present connection is that a large body of facts had accumulated which could be interpreted at least as readily on a Lamarckian basis as on the anti-Lamarckian, granting a mechanism whereby somatic modifications could be transmitted to offspring. The controversy between the two schools to-day centers in the interpretation of hereditary variations initiated by environmental changes, these being interpreted by the Weismannian school on the "induced germinal selection" or parallel induction theory, and by the neo-Lamarckian school on the hypothesis of "somatic induction." It is important to note that the burden of proof in this controversy is conceded to rest on the neo-Lamarckian school.

[1] Thus Weismann: "It is an inevitable consequence of the theory of the germ-plasm, and of its present elaboration and extension so as to include the doctrine of determinants, that somatogenic variations are not transmissible, and that consequently every permanent variation proceeds from the germ, in which it must be represented by a modification of the primary constituents." *The Germ-Plasm*, p. 392.

Let us return to the doctrine of germinal continuity, and define the status thereof at the present time. As aforesaid, Weismann elaborated his theory of germinal continuity on the basis of facts largely brought to light by his researches on Hydromedusæ. Now, Goette [1] has recently gone over much of the ground covered by Weismann's researches on this group, and has shown that the facts support interpretations opposed to Weismann's equally with Weismann's own. Leaving aside the points of subordinate interest, such as the alleged ectodermal origin of the sex cells, Goette shows that there is no warrant for the supposition that the germ cells do not arise through the transformation of material from the germ layer in which they are later seen to lie, which, in a great many cases, be it noted, is the endoderm and not the ectoderm. Weismann himself admitted in his monograph on the Hydromedusæ that "there can be no doubt that the germ cells may reach their differentiation and separation from somatic cells only when the germ layers have long since been formed, and it is impossible to accept as a general law the view of Nussbaum that sex cells are 'absolutely independent of the germ layers.' So far as we can now see, the sex cells always arise in the hydroids from elements of one of the germ layers and they are not merely inclusions in a germ layer but are derivatives, are division products of it." [2]

The force of this argument may appear to be weakened by the fact that, in some species, one of the two cells in the first cleavage of the egg has been identified as a primitive, immature sex cell from which all later germ cells are divided. But when it is considered that the primitive sex cells are by no means identical with or equivalent to mature germ cells, that the latter have a long ontogenetic history behind them, in which the functional activity of the body at large has certainly been involved, and that all germ cells mature by and through their responses to stimuli coming from the outside, it is clear that no very secure theory of germinal continuity can be based on the fact of the early differentiation of sex from other body cells. Germ cells are dependent, the same as all other cells of the body, on water, oxygen and nutritive substance coming from the outside.[3] The nervous system, intercellular bridges, and blood, lymph or other body fluids directly or

[1] A. Goette, "Vergleichende Entwicklung der Individuen der Hydropolypen, *Zeitschr. f. wiss. Zool.*, Bd. 67, 1907.

[2] *Die Entstehung der Sexualzellen bei den Hydromedusen*, p. 284. Cited by Ritter, W. E., *The Unity of the Organism*, Vol. I, p. 68. Our sketch of Weismann's and Goette's researches on Hydromedusæ is based on Ritter, *op. cit.*, Vol. I, pp. 60–68. Cf. also, Romanes, J. G., *An Examination of Weismannism*, pp. 71 ff., 109–110.

[3] Cf. Montgomery, T. H., *The Analysis of Racial Descent in Animals*, pp. 138–141.

indirectly bind all parts of the body together. The germ cells are subject to the processes of metabolism, as are all other living parts of the body.

More specific data demonstrating the dependence of sex glands on other body tissues are supplied by the researches of Miescher,[1] Milroy [2] and others on the chemical changes which occur in the sex glands and other body tissues of some fishes during the reproductive period. Miescher showed, for example, that the "sexual organs in the salmon develop at the expense of the muscular system, and that the salmine deposited in the testis during the breeding season must be derived from the proteins of the muscle, since the fish does not take any food during this period." [3] Finally, Cunningham [4] and others have accumulated evidence indicating the reversibility of the process whereby the gonads determine the development of secondary sexual characters, that is, the exertion by bodily tissues, presumably through hormone action, of a comparable influence on the sex glands.

We have of course cited only a fraction of the evidence bearing on the interdependence of germ and body cells, but enough has been said to show that a theory based on the assumed continuity and independence of the germ-plasm is no longer tenable.

Weismann's subsidiary hypothesis of germinal selection, invoked to explain determinate variation, the degeneration of useless organs and variations generally,[5] has fared just as badly. According to this hypothesis, all variations are due to the struggle of the germinal determinants for nutritive substance, which results in the unequal growth of these determinants, with an exceptional increase of some determinants and the eventual extinction of others. All hereditary changes are thus assumed to be quantitative in nature, being conceived as nothing but plus or minus variations in preëxisting characters. This hypothesis provides no place for the fact that qualitatively new characters appear in the course of phylogenesis. Organic evolution is the emergence, transmission and subsequent modification of new or divergent characters, both qualitative and quantitative, or it is nothing at all. And the qualitative characters are certainly the more significant. Recent work on mutations has fully confirmed this view. Nor does

[1] *Histochemische und physiologische Arbeiten von Miescher, gesammelt and herausgegebene von seinen Freunden,* Bd. II.

[2] "Changes in the Chemical Composition of the Herring during the Reproductive Period," *Biochem. Journal,* Vol. III, 1908, p. 366. See Ritter, *op. cit.,* II, pp. 75–76.

[9] Quoted by *Ritert, loc. cit.*

[4] See Chap. VI.

[5] *The Evolution Theory,* Vol. II, Lectures XXV–XXVI.

experimental evidence bear out the hypothesis of a struggle among parts of the germ-plasm for the nutritive substance therein.[1] Moreover, "if the struggle of the determinants is really an actual and severe one then only those of the large strong organs should survive, all the others being starved out. Such a condition would result in the exclusive development of monsters, *i. e.,* individuals lacking numerous organs (the small ones), and with the large ones all over-developed."[2]

The subsidiary hypothesis of "induced germinal selection," or parallel induction, will be examined at some length later on. To show that this hypothesis does not occupy any superior logical position, compared with the Lamarckian hypothesis, it need only be pointed out that it derives much of its force from the alleged disproof of somatic induction, and that the hypothesis was invented to account for environmentally induced modifications, consistently with the alleged continuity of the germ-plasm and its segregation from the soma-plasm. Nevertheless, the fact that external stimuli can initiate hereditary modifications of the germ cells, without the intervention of the soma, and that parallel induction of changes in soma and germ by means of such stimuli is thus possible in principle, gives to that hypothesis genuine scientific value, and its claims must therefore be considered on their merits. Our contention here is that the hypothesis does not occupy a logically favored position, as its advocates assume.

The deductions based on the theory of germinal continuity and segregation fall to the ground with the disproof of that theory, and the anti-Lamarckian views associated with the theory are placed on the same footing as the Lamarckian theory itself.

The question presents itself why the deductions based on the theory are still so widely accepted, and in a rather uncompromising form, by biologists at the present time.[3] The answer lies, we believe, in two

[1] "Actual experimentation on the influence of food-supply in development does not bear out the assumption on which the theory of germinal selection rests. Weismann himself gave the larvæ of flies, and I have given the larvæ of silkworms through their whole life-time, an abnormally small food supply (in the case of the silkworms this supply was from one-fourth to one-eighth the amount normally eaten by full-fed larvæ), with the only result that the mature individuals were dwarfed; that all their parts were reduced in size, but the actual size proportions of the various organs and parts, and their relations to each other, were unchanged. The determinants seemed to share equally the hardships of short rations rather than a few of the stronger getting the better of the weaker. From the eggs of birds considerable quantities of yolk have been withdrawn without modifying appreciably the individuals developed from the eggs." Kellogg, V. L., *Darwinism To-day,* p. 201; Henry Holt and Company, publishers.

[2] Kellogg, *op. cit.,* p. 201.

[3] The untenability of the theory that the germ-plasm is "continuous" and segregated from the soma-plasm is generally conceded by the Weismannians them-

directions. The rediscovery of Mendel's experiments, and especially of the segregation and independent assortment of the Mendelian factors, the resistance of these factors to somatic influences, the correlation of mutations or Mendelian characters with definite germinal structures, the chromosomes, the regular distribution and orderly behavior of the latter, the demonstrated localization of the pattern and many grosser structures of the body in the egg cells of many species, and, later, the work of Johannsen and others on pure-lines, demonstrating the constancy of the genes generation after generation, in comparative independence of external conditions, all served to rehabilitate the germ-plasm theory, albeit in a modified form, and to render the Lamarckian principle as dubious as in the halycon days of Weismannism itself.[1]

It seems not to have been recognized that the demonstrated stability and orderliness of the germinal factors carried with it absolutely no implications respecting the means whereby such factors are modified, or, in language more conformable to present usage, whereby old factors are dropped out and new factors added. The destructive criticism suffered by the Weismannian theory had really left this an open question, and none of the later developments have in any wise supplied an answer to it. Those developments have been concerned almost exclusively with the size, nature and germinal correlates of hereditary characters, and with the rules or laws according to which those characters appear in the soma, and are transmitted from generation to generation. They have but little to do with the origin, modification or disappearance of hereditary characters. We are not overlooking the experimental evidence which accumulated during this period showing the initiation of hereditary variations by external stimuli. The fact that this was so widely interpreted on a modified form of the germinal hypothesis is only an illustration of the continued predominance of that hypothesis, after the hypothesis itself had been virtually over-

selves. Conklin says: "A germ-plasm which is absolutely distinct from and independent of the general protoplasm is a mere fiction which finds no justification in reality." *Heredity and Environment,* revised second edition, p. 103.

Another Weismannian, J. A. Thomson, writes: ". . . the organism is a unity, and to divide it up, in any hard-and-fast way, into soma and germ cells may land us in the same fallacy as parcelling the mind into separate faculties. It must be admitted, therefore, that it is quite erroneous to think of the germ cells as if they led a charmed life, uninfluenced by any of the accidents and incidents in the daily life of the body which is their bearer." *Heredity,* second ed., pp. 203–4.

Weismann himself, in his later years, really abandoned the central position on which he based his deduction of the impossibility of hereditary somatic modifications, in admitting the inconclusiveness, for this question, of the claim that a transmissionist mechanism is inconceivable. *Evolution Theory,* Vol. II, pp. 110–112, 69–70.

[1] Semon, R., *Das Problem der Vererbung "erworbener Eigenschaften,"* pp. 4–5, and Ritter, W. I., *The Unity of the Organism,* Vol. I, p. 306.

thrown. For a large part of the evidence at least could be equally well interpreted on the hypothesis of somatic induction.

This leads us to the second part of our answer to the question why the deductions based on the theory of germinal continuity and segregation were so largely accepted after the theory itself was virtually exploded, or at least so seriously undermined that it could not possibly support the deductions originally based on it. This part of our answer is admittedly speculative in character, and we offer it, therefore, for what it may be worth. We believe the answer is to be found in the fact that the attention of geneticists came to be centered very largely on the germinal basis of heredity, in an effort to discover structures, substances or mechanisms which could explain the surprisingly definite and orderly phenomena summarized by the Mendelian principles. Attention was so concentrated on this germinal basis of heredity that the inference was made, more or less unconsciously, no doubt, that hereditary characters not only have their seat in the germ-plasm, but that such characters must also originate there. It is significant that statements of biologists respecting the latter question have been largely in the nature of unsupported assumptions that variations do originate in the germ, or, to use a legal phrase, have been mere *obiter dicta* to that effect.

This explanation, if true, is not really surprising. Our minds are so constituted that we tend to magnify the importance of the interests, whether intellectual or otherwise, with which we happen to be associated, and to claim more for them than they are entitled to. Just as almost every important social factor has been made the foundation of a system of sociology, so biologists, being human, have constructed systems of biology on the basis of one or a few significant general facts regarding living organisms. Germinal theories respecting the origin of variations rest on similar foundations, we believe, as, for that matter, do the extreme forms of the Lamarckian theory, and indeed all other theories based on a *selection* from the significant factors involved in vital phenomena.

We should have to add to this proffered explanation, and as partially explaining the explanation itself, the transference of habits of thought associated with the Weismannian theory to the period after the theory had been overthrown by the criticism directed against it. Tradition and habit die hard everywhere, the domain of science included. How else could we explain the positive conviction in the later period, that somatic modifications are not heritable, when the logical foundations for the belief had been destroyed, and a large body of facts had

been accumulating which were fully as consonant with the opposing hypothesis?

Nevertheless, the conclusions based on the doctrine of germinal continuity *may be* true, though the doctrine itself be false. In other words, it is *conceivable* that hereditary variations arise in the germ-plasm from causes at present unknown but discoverable at some future time. It is *not* conceivable that the germ-plasm is immune from somatic influences; yet the hereditary variations having their seat and possibly their origin in the germ-plasm may not be representative in any strict sense of processes going on in the body at large. The functional activity which is always present in vital phenomena, whether germinal or somatic, may in this situation be purely instrumental, and without any additional significance for the origin of hereditary variations. It may be only the functional activity of the germ-plasm itself which takes an active and positive part in phylogenesis. In short, according to the usual way of viewing the problem, the germinal origin of hereditary variations is quite conceivable.

It is possible, indeed, to *interpret* on this theory virtually all the evidence adduced in support of the Lamarckian principle. And the opponents of the Lamarckian principle do, as a matter of fact, call in question the evidence adduced in support of that principle, by proceeding to interpret this evidence on the germinal and other anti-Lamarckian hypotheses.[1] And this procedure is deemed satisfactory for the reason that, due to psychological causes already indicated, the Weismannian hypothesis is assumed to be valid to start with. This procedure will

[1] See, for example, Thomson's chapter on "The Transmission of Acquired Characters," *Heredity,* second edition, pp. 164–249. It should be stated that Thomson is one of the less dogmatic of the Weismannian school. Plate writes, in discussing selectionist apologetics (for it may rightly be termed that): ". . . ein extremer Neodarwinist wird stets den Verdacht auf Atavismus hegen oder er wird vermuten, der Originalreiz sei nicht richtig erkannt worden und die wirkliche Ursache der Veränderung habe in beiden Generationen ihren Einflusz auf das Soma ausgeübt oder endlich, er wird sagen, die betreffende Eigenschaft sei zwar neu, aber sie stehe in keiner Beziehung zu dem angeblichen Originalreiz, sondern sei von vornherein durch eine zufällige Veränderung des Keimplasmas bedingt worden (vgl. Semon, 1907, S. 5 ff.) Hat man alle diese Moglichkeiten entkräftet, so wird er den Begriff der Vererbung erworbener Eigenschaften vielleicht noch willkürlich einengen auf die funktionellen Reizwirkungen, um auf diese Weise die Position zu halten." *Selectionsprinzip und Probleme der Artbildung.* 1908 ed., pp. 326–327. (The work of Semon referred to is "Beweise f. d. Vererbung erworbener Eigenschaften, ein Beitrag zur Kritik der Keimplasmtheorie," *Arch. f. Rassenbiol.,* Vol. IV, pp. 1–46.) Semon claims, in another work, that critics of the Lamarckian hypothesis seize on this or that weak point in experiments yielding results favorable to the hypothesis, and then claim that there is no valid experimental (einwandfreie) evidence in support of the hypothesis. *Das Problem der Vererbung "erworbener Eigenschaften,"* pp. 172–73.

no longer serve, once it is recognized that the two hypotheses are at the present time on a substantially equal footing, so far as the experimental evidence is concerned. Were the traditional positions of the two hypotheses reversed, the neo-Lamarckians could readily interpret on their hypothesis much if not most of the evidence which might be brought forward in support of the Weismannian hypothesis.

When the burden of proof is equally divided between the two hypotheses, the impasse between them seems to be complete, provided the Weismannians be allowed to avail themselves of the parallel induction theory. For there are large bodies of facts which, according to the present way of viewing the problem, may be interpreted passably well on either hypothesis. That fact is the most significant feature of the present situation. When a vast array of facts can be plausibly interpreted on two opposing hypotheses, there is ground for the suspicion that the issue between them rests on unsound logical foundations. This suspicion is deepened by the fact that advocates of either hypothesis are confronted by evidence which they find very embarrassing, or which they are unable to use. When each of two opposed hypotheses is able to account satisfactorily for many of the facts bearing on both hypotheses, but not for all of them, it is likely that there is an admixture of truth and error in both hypotheses. Our further examination of the problem will, we believe, bear out this view of the situation.

Let us complete our preliminary analysis of the germinal theory of variations. The more positive part of our criticism, to be presented later, need not depend, for its validity, on the outcome of current controversies regarding the part played by germinal structures and substances in heredity. Our analysis will have all needful reference to these questions, but its validity will not be affected by the solutions thereof. The analysis will also have reference to divergent views respecting the genesis of variations in the germ-plasm, but our argument is also independent, in its general form, of the particular views on this question.

It has been fully demonstrated that definite parts of the egg cell, in some animals at least, represent the general pattern and grosser structures of the embryo, and of the adult organism.[1] Moreover, all the evi-

[1] For examples of such localization see Conklin, E. G., "The Organization and Cell-Lineage of the Ascidian Egg," *Jour. Acad. Nat. Sci. Phila.*, 2d series, Vol. XIII, 1905, pp. 5-119; and Wilson, E. B., "Experimental Studies on Germinal Localization. I. The Germ-Regions in the Egg of Dentalium," *Jour. Exp. Zool.*, Vol. I, 1904, pp. 1–72, and "Experimental Studies on Germinal Localization. II. Experiments on the Cleavage-Mosaic in Patella and Dentalium." *Ibid.*, Vol. I, 1904, pp. 197–268.

dence goes to show that the germ cell as a whole is constituted of specific structures and substances having a specific distribution and behaving in definite ways throughout the period of ontogenesis. Although many of the grosser bodily structures are apparently derived from substances definitely localized in the egg cells, the work on Mendelian inheritance and its chromosomal basis has shown pretty definitely that a single "gene" may affect a number of bodily characters, and that a number of genes may affect the same bodily character. The suggestion has been made, indeed, that every part of the body may be influenced by the entire germ-plasm.[1] In any case the discovery that a number of genes sometimes affect the same body part[2] invalidates the view of Roux and Weismann "which conceived each end result as the special product of one or a few particular genes."[3]

It is not essential to our purpose that we should attempt to reconcile the fact just cited with the equally indisputable fact that the germinal elements most conspicuously involved in the development of many body parts have been definitely localized (for some animals) in the cytoplasm of the egg cells. Those elements may and probably do develop, in part, by and through the interaction of the cytoplasm with the nucleus, including the chromosomal material therein. At all events "we are justified in speaking of the particulate composition of the germ-plasm and of particulate inheritance."[4] We will add that the structures and substances of the germ-plasm, whatever their nature, must be definitely organized, and that their behavior during development must also be organized. We could not, on any other assumption, understand the orderly processes of development, and the definite results into which they issue.

For our purposes we may loosely summarize the facts in the case by saying that each character of the body is produced by a greater or smaller number of genes or factors in the germ-plasm acting in an orderly, coöperative manner. We need affirm nothing as to the combinations and transformations of these factors in ontogeny, or as to the rôles of the cytoplasm, nucleus and chromosomes, respectively,

[1] Morgan, T. H., "Concerning the Mutation Theory," *Sci. Mo.*, Vol. VI, 1918, p. 404; and *The Physical Basis of Heredity*, p. 240.

[2] Morgan, *The Physical Basis of Heredity*, p. 240.

[3] Morgan, T. H., "Concerning the Mutation Theory," *loc. cit.* ". . . it would be well," says E. B. Wilson, "to drop the term 'determiner' or 'determining factor' from the vocabulary of both cytology and genetics. What we really mean to say is 'differential' or 'differential factor,' for it has become entirely clear that every so-called unit character is produced by the coöperation of a multitude of determining causes." "The Bearing of Cytological Research on Heredity," *Proc. Roy. Soc.*, Vol. LXXXVIII. 1914, p. 351.

[4] Morgan, T. H., *The Physical Basis of Heredity*, p. 243.

in the development of the individual or of the germ cell itself.

Three fairly distinct hypotheses respecting the causes of variations in the germ cells have been advocated by adherents of the germinal theory. One of these is the hypothesis elaborated by Weismann, Roux and others, that variations are due to some sort of struggle among the parts of the germ cell for food and position, resulting in the unequal growth of these parts, with the accelerated growth of some parts and the degeneration or eventual extinction of other parts. Weismann largely attributed germinal variations to the fluctuations of nutritive substance in the germ-plasm. He ascribed more or less importance to amphimixis, however, claiming that it results in the combination of single variations which cannot be acted on by natural selection, if taken separately, because of their non-adaptive character, but which can be if combined through amphimixis into adaptive characters.[1] This whole process of variation, with combination of variations through amphimixis, is conceived to be under the ultimate control of personal or natural selection. According to Weismann's conception variations are only quantitative in nature, although the accumulation of these issues in differentiations ordinarily termed qualitative.[2] Induced germinal selection is the subsidiary hypothesis on which Weismann explains, consistently with his general theory, hereditary variations stimulated by environmental changes.

Weismann's system could scarcely be regarded as materialistic in the strict sense of the term, for the ultimate hereditary units assumed are the biophors, which are represented as *organizations* of chemical molecules, and as exhibiting the "primary vital forces, viz., *assimilation and metabolism, growth, and multiplication by fission.*"[3] Weismann's conception of the organism is thus morphological, not chemical, in character.[4] It might be said to stand between the physicochemical and vitalistic conceptions of life, assuming as it always does, vital units not equivalent to chemical bodies, though attributing changes in such units to physical or chemical agencies. The various kinds of vital units assumed lead a semi-independent existence, all of them exhibiting the "primary vital forces" of assimilation, metabolism, growth and reproduction.

[1] *The Germ-Plasm,* p. 432; *Ev. Theory,* Vol. II, pp. 194 ff.

[2] Weismann at times entertained the hypothesis that variations in the biophors could arise from changes in the chemical constitution of the constituent molecules, or in a rearrangement of the molecules. (See *The Germ-Plasm,* pp. 43–44.) Variations so caused would be interpreted as qualitative in character. The theory in its final form, however, represents variations as quantitative in character.

[3] *The Germ-Plasm,* pp. 39–40.

[4] See *The Germ-Plasm,* pp. 38–39.

Since the experimental evidence regarding the effect of limited food on the germ cells contradicts Weismann's assumption that germinal variations are due to fluctuations of nutritive substance, and since the doctrine of germinal continuity and segregation in its strict form has been overthrown, we might consider the theory sufficiently refuted without adducing other evidence against it. But, as we have shown, it may be claimed that hereditary variations originate in the germ-plasm even though the germinal theory in its original form has been discredited. The composition, constitution and capacities of the germ-plasm may be such that hereditary changes could originate there with the aid of very general means, such as nutrition, supplied by the body at large. According to this conception, hereditary variations would be attributed to a quasi-vital process, or at least a process not interpreted explicitly in chemical terms. This version of the germinal theory may be contrasted with the two versions now to be described.

One of these versions maintains that the appearance of new genes or factors in the germ-plasm, as well as the disappearance (or modification) of preëxisting factors, is simply a chemical process, and that all factors or genes are "some sort of chemical bodies." This is the typical mutationist view.[1]

It will be convenient to examine this version of the germinal theory in connection with the general physicochemical conception of the organism, of which it is an integral part. Insofar as this hypothesis represents a distinct contribution to anti-Lamarckism, however, it will be considered concurrently with other anti-Lamarckian hypotheses, when we come to apply our positive tests thereto.

The mutation and genotype conceptions of the organism assume the correctness of the chemical hypothesis just indicated, and are not differentiated from each other in regard to this matter. The distinctive contribution of the genotype theory consists not in any essentially new hypothesis respecting the causes of variations, but in its sharp distinction between pure-lines and more or less heterogenous populations comprehended by the same species or variety. This certainly had its influence on current views regarding the nature of the germinal factors, but it scarcely affected the prevailing hypotheses respecting the causes of changes therein, except perhaps by defining them more sharply than before.

The vitalistic theory of phylogenesis, as represented by H. Bergson, may be regarded as a third form of the germinal theory respecting the

[1] See T. H. Morgan, "Changes in Factors Through Selection," *Sci. Mo.*, Vol. VI, 1918, p. 555; "Concerning the Mutation Theory," *Sci. Mo.*, Vol. VI, p. 394; and Jennings, H. S., "Organic Evolution," *The American Year Book*, 1918, Sec. XXIV.

origin of variations. Vitalistic theories concerned primarily with the interpretation of ontogenesis sometimes imply the same type of anti-Lamarckism, as does Driesch's theory.[1] These theories incorporate the dominant views regarding the Lamarckian principle, and contribute little or nothing distinctive to anti-Lamarckism as such. Their examination may therefore be postponed to later chapters altogether, where this feature will be considered in relation to the systematic conceptions of life set forth in those theories.

The Hypothesis of Parallel Induction

We turn now to our preliminary examination of the hypothesis that certain classes of hereditary variations stimulated by environmental changes represent a species of direct action on the germ-plasm, and not functional activity in any creative sense. This hypothesis was originally propounded by Galton,[2] later adopted by Cope,[3] under the term Diplogenesis, and added by Weismann as a subsidiary hypothesis to his germ-plasm theory, under the rubric of "induced germinal selection." It was rechristened in 1904 by Detto as the hypothesis of parallel induction,[4] the designation now most commonly applied to it.[5] This hypothesis is to the effect that somatic modifications demonstrably induced by environmental changes, and repeated in a succeeding generation or generations without the original stimulating cause being present, do not themselves affect the germ-plasm, but that both the somatic and the germinal changes are produced concurrently by the same stimulus. Hence the term, parallel induction.

The hypothesis is usually restricted in its application to modifications which could either be classed as passive adaptations, or as non-adaptive characters. It is, however, sometimes employed to account for modifications of active adaptations traceable to specific environmental changes.[6] But the great bulk of active adaptations are interpreted by the anti-Lamarckians on some form of the germinal hypothesis, properly so-called. Where this hypothesis does not serve, however, the germinalist can fall back on the parallel induction hypothesis. For this reason the controversy between Lamarckians and anti-Lamarck-

[1] See Chap. XIV.

[2] "A Theory of Heredity," *Cont. Rev.*, Vol. XXVII, 1875, p. 92.

[3] "On Inheritance in Evolution." *Am. Nat.*, Vol. XXIII, 1889, pp. 1058–1071.

[4] *Detto, C., Die Theorie der direkten Anpassung*, pp. 199 ff.

[5] The doctrine of "direct" environment action was accepted by Lamarck, Darwin, Spencer, Haeckel, Eimer and others of the older biologists, though not in the form here under consideration.

[6] See Thomson's discussion of the changes in reproductive habits of the black salamander produced in Kammerer's experiments. *Heredity*, second ed., pp. 215–216.

ians now centers around this issue. So far the Lamarckians have not been able to repulse their opponents from the position taken in this controversy, though the latter have been obliged, from time to time, to repair breaches in the defenses which they have erected around it.

Two bodies of evidence support the hypothesis under consideration. There is, first, a vast amount of experimental evidence demonstrating the possibility of inducing hereditary variations by subjecting germ cells to external stimuli, including various chemical substances, X-rays, changes in temperature or humidity, etc. The inference was drawn from this evidence that germ cells could be similarly affected by such stimuli when lodged in the body of the parent organism. And there is little doubt that alcohol and various other poisons do have such an effect. But this evidence by no means proves that the hypothesis of parallel induction supplies a tenable interpretation of the cases in dispute between the advocates of this and of the opposing hypothesis.

The second body of evidence supporting the hypothesis, and this of a more convincing character, was yielded by Tower's experiments on Leptinotarsa. The reported results of these experiments apparently proved that hereditary variations experimentally produced in this genus represented direct responses of the germ cells to external stimuli, and that such variations could not be accounted for, therefore, on the hypothesis of somatic induction.[1] The influence of Tower's work was enormous, and apparently convinced the majority of biologists that functional activity does not play a causal rôle in the genesis of variations induced by environmental changes.[2] However, certain private reservations by Tower in reporting his results have cast doubt on the reliability of the work as a whole, and it now figures less in discussions of the problems affected than it did formerly.[3] Moreover, Semon has published a very acute analysis of Tower's results (taking them at their face value), which shows that they may be interpreted on the hypothesis of somatic inductions.[4] This body of evidence cannot be taken, therefore, as substantiating the hypothesis of parallel induction.

We need not enter into a detailed account of the controversy between the advocates of these opposing hypotheses. It will suffice to show by a presentation of the more significant evidence bearing on the controversy that the impasse between the two hypotheses is all but

[1] Tower, W. C., *An Investigation of Evolution in Chrysomelid Beetles of the Genus Leptinotarsa,* Chaps. III, V. The Lamarckian principle is often referred to in the literature on the present topic, as somatic induction.

[2] Semon, *erworbener Eigenschaften,* p. 105.

[3] See Bateson, W., *Problems of Genetics,* pp. 219–227.

[4] *Op. cit.,* Chap. VIII.

complete and that a new approach to the problem is necessary if we are to make any progress toward its solution.

There is some evidence that hereditary modifications have been induced by environmental stimuli which are not explicable on the hypothesis of parallel induction, but this evidence taken by itself is not very convincing. We shall cite a part of this evidence in order to illustrate the inconclusive character of the affirmative argument for the rival hypothesis.

Schröder [1] compelled certain moths (*Gracilaria stigmatella*) which build their houses by rolling themselves, flutelike, into the ends of willow leaves, to utilize the sides of the leaves for this purpose. This was done by cutting off the tips of the leaves accessible to them. The second generation were compelled to build their houses in the same way. The third generation (19 individuals) were brought up normally, and four of the nineteen followed the new mode of building their houses, in preference to the mode long followed by their ancestors. Parallel induction is here clearly ruled out, for the new mode of house-building could not have been produced "directly" by the stimulus (the clipped willow-leaves) acting on the germ-plasm. It was apparently a *somatic response* to the stimulus which affected the germ-plasm. Yet this evidence is not very convincing, because the number of individuals exhibiting the new mode of response was very small,[2] and it is possible that such variations occur under the artificial conditions of captivity, if not under normal conditions.[3]

The same general considerations apply to the experimentally induced changes in the egg-laying habits of the beetle, *Phratora vitellinae*, as reported by the same investigator. Beetles of this species were transferred for three successive generations from willows with smooth leaves (*Salix fragilis*) upon which the beetles normally feed and lay their eggs, to a willow (*S. Viminalis*) having the under side of the leaves felted. The individuals of the fourth generation all made their ovipositions on leaves of the felt-leaved willow, although given a free choice between trees of the two species.[4] While these results cannot be interpreted on the hypothesis of parallel induction, since the inducing stimulus could not have affected the germ cells directly, the limited number of ovipositions (15) by individuals of the fourth generation, together with certain conditions attending the experiment, make the

[1] Schröder, C., "Über experimentell erzeugte Instinktvariationen," *Verhandl. d. Zool. Ges. Leipzig*, 1903.

[2] See Semon, *op. cit.*, p. 121.

[3] Bateson, W., *Prob. of Gen.*, pp. 194–195.

[4] Schröder, *op. cit.*

evidence less cogent for the opposing hypothesis than it otherwise would be.[1]

Of the same order are results reported by Pictet.[2] This experimenter forced caterpillars (*Limantria dispar*) to eat a different food (fir-needles) from that to which they were accustomed (oak leaves, etc.) and to attack this food in a different way (at the tips of the leaves instead of the sides), despite great difficulties due to the structure of their mandibular apparatus. Individuals of the second generation exhibited the new feeding habits without compulsory measures being applied to them. The only reasonable interpretation of this case is supplied by the hypothesis of somatic induction, since it was clearly a somatic response to the stimulus which became hereditary. Yet the evidence is not very cogent, owing to the small number of individuals exhibiting hereditary modifications of the feeding responses, although control experiments were included in the investigations reported in 1911.[3]

We may summarily cite as evidence susceptible of the same interpretation the old experiments of Marie v. Chauvin on the Mexican Axolotl,[4] whereby the latent tendency of this salamander to metamorphose into a lung-breathing animal was, through changed environmental conditions, much accentuated; Kammerer's experiments on the midwife toad (*Alytes obstetricans*),[5] whereby, through heightened temperature, certain hereditary modifications in the reproductive processes and structures peculiar to this species were induced; experiments of the same biologist on *Salamandra atra* and *maculosa*, whereby, through a change in environmental conditions, striking hereditary modifications in the processes of reproduction were induced,[6] including

[1] Bateson, W., *op. cit.*, pp. 193–194; Semon, *op. cit.*, p. 121.

[2] "Influence de l'alimentation et de l'humiditié sur la variation des papillons," *Mem. de la Soc. de Physique et d'Hist. nat. de Genève*, Vol. XXXV, 1905; "Quelques exemples de l'héréditié des caractères acquis," *Verh. Schweiz. nat. Ges.*, Vol. I, 1910; "Un nouveau exemple de l'héréditié des caractères acquis," *Arch. sci. phys. et nat.*, Vol. XXXI, 1911. Summarized by Semon, *op. cit.*, pp. 81–82.

[3] See Semon, *op. cit.*, p. 121.

[4] "Ueber die Verwandlung der mexicanischen Axolotl in Ambystoma. *Zeit. f. wiss Zool.*, Vol. XXVII, 1876; "Uber die Verwandlungsfähigkeit des mexikanischen Axolotl," *ibid.*, Vol. XLI, 1885.

[5] "Experimentelle Veränderung der Fortpflanzungstätigkeit bei Geburtshelferkröte (Ayltes obstetricans) und Laubfrosch (Hyla arborea)," *Arch. f. Entwm.*, Vol. XXII, 1906; "Vererbung erzwungener Fortpflanzungsanpassungen. 3. Mitteilung. Die Nachkommen der nicht brutpflegenden Alytes obstetricans," *ibid.*, Vol. XXVIII, 1909; "Vererbung erzwungener Farb- und Fortpflanzungsveränderungen," *Natur*, Heft 6, 1910.

[6] "Beitrag zur Erkenntnis der Verwandtschaftsverhältnisse von Salamandra atra und maculosa," *Arch. f. Entwm*, Vol. XVII, 1904; "Vererbung erzwungener Fortpflanzungsanpassungen. 1 u. 2. Mitellung. Die Nachkommen der spätgeborenen

an increase or decrease in the number of offspring born (according to the species subjected to the changed conditions) and a shifting forward or backward, of their stage of development at birth.[1]

Kammerer's experiments have aroused a good deal of controversy, and the judgments expressed regarding them are varied and conflicting. Into the details of this controversy it is unnecessary for us to enter. Insofar as modifications of hereditary adaptations to the environment were induced through changed environmental conditions, Kammerer's results must be interpreted on the Lamarckian hypothesis. We cannot conceive a "direct" modification of germinal factors or determinants (whether located in the gonads or in body tissues) which could determine, in later generations, copulation in water instead of on land, or *vice versa,* a reduction or increase in the number of offspring produced, or a shifting forward or backward of the stage of development at birth. All these are active somatic responses induced by and adapted to changed environmental conditions, and we shall have to postulate some sort of preëstablished harmony between somatic and germinal modifications if we suppose that the changed conditions induce modifications in the germ cells which determine, in later generations, behavior of the same type as that induced in the parent organisms by the same conditions. The development of these considerations must be postponed, however, to a later part of the discussion.

The Lamarckian interpretation of these results, granting their authenticity, is scarcely affected by the probability that the changes in question represented only the emergence of latent characters, and not the genesis of new ones.[2] Unquestionably modifications of hereditary characters were induced by the changed conditions, and these modifications were in the nature of responses better adapted to the new conditions; those modifications must therefore have phylogenetic significance, even though they consist wholly in the submergence of active characters and the emergence of others already existent in a latent form. The same considerations apply to Fr. Chauvin's experiments on the Mexican Axolotl, assuming the genuineness of the results re-

Salamandra maculosa und der frühgeborenen Salamandra atra," *ibid.,* Vol. XXV, 1907.

[1] We are following the summaries of Bateson and Semon in our account of Kammerer's experiments. See Bateson, W., *Problems of Genetics,* pp. 199 ff., and Semon, R., *erworb. Eigensch., passim.* It is not our purpose to enter into a critical examination of this and similar evidence supporting the hypothesis of somatic induction, as we are doubtful of any very decisive results being attained through present methods of dealing with such evidence; and our own method of dealing with the problem, to be developed later, will be employed in relation to evidence of a less doubtful character.

[2] Plate, L., *Selectionsprinzip,* 1908 ed., p. 429.

ported. The results from Kammerer's and Chauvin's experiments are of doubtful value, not because, as reported, they fail to yield significant conclusions for the problem in hand, but because the results themselves are open to question. For this reason we cannot claim them as yielding a decisive proof that functional activity has a specific effect on hereditary modifications.[1]

More doubtful must be the interpretation of induced hereditary modifications whose genesis is not clearly associated with active somatic responses on the part of the parent organisms. Perhaps all such modifications would be classifiable either as passive adaptations or as characters having no adaptive value. Functional activity certainly plays a rôle in the genesis of such modifications, but we cannot claim that it exerts a specific or representative influence thereon.

Before considering such modifications, we may refer briefly to a curious case which apparently stands on the borderline between modifications most reasonably interpreted on the hypothesis of somatic induction, and modifications to which the hypothesis of parallel induction can be plausibly applied. The work of Semon, Pfeffer and other biologists [2] has pretty conclusively demonstrated that the "sleep movements" (Schlafbewegungen) of plants represent, in part, the inheritance of modifications induced by the environment—the alternation, by day and night, of periods of light and darkness. The opening and closing of leaves and flowers in the plants studied by these investigators were found to occur at twelve-hour intervals, when such plants were raised and kept in continual darkness or light. This clearly demonstrated the continuing influence of previous environmental conditions on hereditary characters. Confirmatory of this conclusion was the further discovery that these periodical movements could be modified by light-and-darkness in alternations different from the day-and-night alternations, as by six-, three- or twenty-four-hour intervals between

[1] Regarding the validity of Kammerer's results, see Jennings, H. S., *Life and Death. Heredity and Evolution in Unicellular Organisms*, pp. 202–3. A résumé of evidence from Kammerer's and other experiments bearing on the Lamarckian hypothesis is presented in a recent book by Kammerer himself, *The Inheritance of Acquired Characteristics.*

[2] Semon, R., "Über die Erblichkeit der Tagesperiode," *Biol. Centralbl.*, Vol. XXV, 1905; "Hat der Rhythmus der Tagezeiten bei Pflanzen erbliche Eindrüke hinterlassen?" *ibid.*, Vol. XXVIII, 1908; Pfeffer, W., Untersuchungen über die Entstehung der Schlafbewegungen der Blattorgane," *Abhandl. d. math.-phys. Kl. d. kgl. sächs Ges. d. Wissensch.*, Vol. XXX, 1907; "Der Einflusz von mechanischer Hemmung und von Belastung auf die Schlafbewegungen," *ibid.*, Vol. XXXII, 1911; Stoppel, R., "Über den Einflusz des Lichtes auf das Öffnen und Schlieszen einiger Blüten," *Zeitschrift. f. Botanik.* 2. Jahrg., 1910; Stoppel, R. and Kniep, H., "Weitere Untersuchungen über das Öffnen und Schliezen der Blüten," *ibid.*, 1911. Summarized by Semon, *erworb. Eigenschaft.*, pp. 18–21.

the two, or by regular cycles with unequal periods of light and darkness.

The favored explanation of this case is supplied by the hypothesis of parallel induction, and the neo-Lamarckians do not appear to have demurred against it. This is probably because the criterion adopted by the latter for cases of somatic induction allows for the *possibility* of parallel induction, when the inducing stimulus is not clearly excluded from the germ cells, or is of such a nature, qualitative or quantitative, that it could be conceived capable of modifying them. But it seems to us that the criticism directed against this interpretation of previous cases applies here also. The opening and closing of leaves or blooms, in correspondence with alternating periods of light and darkness, represent *active* adaptations to these stimuli, and we fail to see how light and darkness could so modify the germ-plasm directly that it could determine opening and closing movements similar to those of the parent organisms. For such a process must involve a pre-established harmony between the somatic and (alleged) germinal modifications induced by the stimulus in question. As the facts in these cases are incontestable, we should be disposed to claim for them greater significance for the problem in hand, than for any of the cases previously cited.

Another borderline case, as it were, is Cunningham's experiment on flatfishes.[1] This experimenter induced the formation of pigment on the under side of flounders, which are normally unpigmented, by lighting from underneath the glass vessel containing them, light from above being excluded. The absence of pigment from the lower side of these fishes is proved to be a hereditary character by the fact that it required several weeks of continuous lighting to induce the formation of pigment on that side, whereas the upper side was fully pigmented during this period. It was maintained by Cunningham, and later by Plate,[2] that the disappearance of pigment from the lower side of these animals could not be explained on the hypothesis of accidental germinal variations; Cunningham arguing that if due to former germinal variations, the application of the light stimulus later could not cause the pigment to reappear; while Plate pointed out that, since some species of flatfish lie on the right side, and others on the left side, accidental variations in the germ-plasm would not always correspond to the resting positions taken by the several species of this group.

[1] An Experiment concerning the Absence of Color from the Lower Sides of Flat-fishes," *Zool. Anz.*, Vol. XIV, 1891, pp. 27–32; "The Problem of Variation," *Nat. Sci.*, Vol. III, 1893, pp. 285 ff.

[2] *Selectionsprinzip*, 3d ed., pp. 344–346.

Plate has also maintained that the hypothesis of parallel induction is not applicable to this case. His argument is that, if the light penetrates to the ovaries from the upper side, and darkness from the lower side, one of the ovaries should yield completely pigmented, the other unpigmented, offspring.[1] This argument is hardly a conclusive one, as it does not take into consideration the hypothesis, accepted by Plate in other applications, that the somatic tissues through the processes of ontogenesis carry their germinal determinants with them, that these may therefore be directly modified by external stimuli, and pass on these modifications to corresponding determinants in the germ cells. This case could be interpreted on the hypothesis of parallel induction, when associated with this version of the germinal theory. We shall postpone our observations respecting the interpretation of such cases on the hypothesis of parallel induction, until other cases of the same sort have been reviewed. As aforesaid, we cannot in these cases claim any representative influence for functional activity as a factor in phylogenesis.

In the same general category are to be included Fischer's experiments on *Arctia caja,* in which hereditary aberrations of the color pattern were induced by subjecting the pupæ to extreme cold; [2] and the similar experiments of Schröder [3] on *Abraxas grossulariata,* in which strongly marked melanistic aberrations were produced by the abnormal temperature, these modifications appearing in a number of the offspring raised under normal conditions. An interesting feature of Schröder's work was his exact measurements of the modifications induced by the temperature stimulus, and the presentation of the results in statistical form. Since these experiments present nothing new in principle, as compared with other cases already cited, we need not enter into the details.[4]

Similar in principle is Kammerer's experiment on *Proteus anguinus,* a dweller in dark holes, and virtually devoid of pigment. By keeping individuals of this species in the light, deposition of pigment in the form of brown and blue-black spots was induced, and this pigmentation appeared in offspring raised under normal conditions. Of special interest was the appearance of pigmentation in the young when the

[1] *Op. cit.,* p. 346.

[2] Fischer, E., "Experimentelle Untersuchungen über die Vererbung erworbener Eigenschaften," *Allg. Zeitschrift. f. Entomologic,* Vol. VI, 1901, pp. 49, 363, 377.

[3] "Die Zeichnungsvariabilität von Abraxas grossulariata," *Allg. Zeitschrift. f. Entomologie,* Vol. VIII, 1903.

[4] Plate, *op. cit.,* pp. 337–340, presents an interesting interpretation of Fischer's results.

father but not the mother had been subjected for the requisite period of time to the light stimulus.[1]

Kammerer also induced abnormally dark coloring in lizards of three different species (*Lacerta serpa, oxycephala* and *muraliso*) by keeping them in a very hot dry room. Eggs of these individuals laid in a cool room and hatched there produced young which were normally colored at first, but later developed a darker color. The coloring gradually became normal under the usual conditions of temperature and moisture.[2]

Two other series of experiments are of special interest because of their relation to later subsidiary experiments designed to test the *possibility* of parallel induction of modifications in germinal and somatic tissues. One group of experiments, conducted by Sumner and Przibram, were designed to determine the effects of abnormal temperature stimuli on warm-blooded animals (mice and rats in these experiments). The other group is Kammerer's well known experiments on the induction of color changes in the black-and-white salamander.

Sumner [3] raised one set of white mice in a warm room (medium temperature, 26.3°C.) and another set in a cold room (med. temp., 6.1° C.), thereby inducing various bodily changes partially transmissible to offspring raised under normal conditions. The ears, tail and feet of the mice kept in the warm room were from twelve to thirty per cent longer at maturity than the same parts of mice raised in the cold room, while the pelts of the latter weighed on an average 13.6 per cent more than those of the former, and had on them a larger number of hairs. Mice kept in the respective rooms were paired, after which the females were taken to rooms of medium temperature, and their offspring later compared, with the aid of exact measurements. The body weight of offspring from mice raised in the warm room was found to be less than that of offspring from mice raised in the cold room, while the ears, tail and feet of the former were slightly, though measurably, longer.

[1] Kammerer, P., "Experimente über Fortpflanzung, Farbe, Augen und Körperreduktion bei Proteus anguinus Laur. III. Mitteilung über Vererbung erzwungener Farbveränderungen," *Arch. f. Entwm.*, Vol. XXXIII, 1912. See Semon, *erworb. Eigensch.*, p. 76.

[2] "Zuchtversuche zur Abstammungslehre," in *Die Abstammungslehre*, 1911; "Vererbung künstlicher Zeugungs- und Farbenveränderungen," *Die Umschau*, 1911; "Direkt induzierte Farbenanpassungen und deren Vererbung," *Zeitschrift f. induktive Abstammungs- u. Vererbungselhre*, Vol. IV, 1911. See Semon, *op. cit.*, p. 77.

[3] Some Effects of External Conditions upon the White Mouse," *Jour. Exp. Zool.*, Vol. VII, 1909, pp. 97–155. "The Reappearance in the Offspring of Artificially Produced Parental Modifications," *Am. Nat.*, Vol. XLIV, 1910, pp. 5–18; "An Experimental Study of Somatic Modifications and Their Reappearance in the Offspring," *Arch. f. Entwm.*, Vol. XXX, Part II, 1910, pp. 317–348; "Some Effects of Temperature upon Growing Mice, and the Persistence of Such Effects in a Subsequent Generation," *Am. Nat.*, Vol. XLV, 1911, pp. 90–98.

Przibram [1] experimented on rats by keeping them in a room much hotter than Sumner used for his experiments, the temperature being maintained at 30° to 35° C. This treatment resulted in a reduction of hair, the enlargement of the free body parts, and an especially striking increase in the size of the external genitalia. A series of generations were raised under these abnormal temperature conditions. It was found that the induced modifications (Hitzemerkmale) appeared spontaneously in the fourth generation, that is, in the absence of the abnormal heat stimulus. The mother of these F4 individuals had been placed after conception in a cooler temperature, and the young were raised under these conditions.

Kammerer has reported that the color markings of the black-and-yellow salamander (*Salamandra maculosa*) can be greatly modified by the application of certain stimuli, and that these changes are transmitted in a somewhat modified form to offspring.[2] By keeping the salamander for a considerable period of time on yellow clay soil, the yellow spots increased in size and number at the expense of the black ground color. The reverse happened when the animals were kept for a somewhat longer period on black garden soil, the coat color becoming almost black.

These modifications appeared in offspring raised under normal conditions. The results were most striking in the case of offspring from animals kept on yellow soil; the yellow was larger in amount than normal, even when the young were raised on black soil. The stimuli in all these cases are considered to have included not only the changed ground color, but moisture from the earth as well. Special experiments showed that the yellow spots of blinded animals kept on yellow soil increased in number but not in size. A special point of interest for our later discussion is that the eye was shown to be the stimulus-receptor for part of the modifications induced in all the animals used in this experiment. It is to be noted, however, that the light stimulus still affected the epidermis of the blinded animals, and together with the moisture stimulus induced the changes (increased number of yellow spots) occurring therein. No attempt was made in these experiments

1 "Versuche an Hitzeratten," *Verhandl. der Ges. deutscher Naturf. u. Ärzte*, Versammlung Salzburg, 1909.

2 "Vererbung künstlicher Farbenveränderungen," *Die Umschau*, 13 Jg., 11 Dez. 1909; "Zuchtversuche zur Abstammungslehre," in *Die Abstammungslehre*, 1911; "Direkt induzierte Farbenanpassungen und deren Vererbung," *Zeitschrift f. induktive Abstammungs- u. Vererbungslehre*, Vol. IV, 1911; "Vererbung erzwungener Farbveränderungen. IV. Mitteilung: Das Farbkleid des Feuersalamanders (Salamandra maculosa Laurenti) in seiner Abhängigkeit von der Umwelt," *Arch. f. Entwm.* Vol. XXXVI, 1913, pp. 4–193.

to determine the effects of light and moisture stimuli, respectively, on the offspring.

Interesting experiments have been undertaken with a view to testing the possibility of a simultaneous induction of modifications in germ and body cells through operation of the same stimulus. These experiments may be regarded as auxiliary to experiments for determining the effects of light and temperature stimuli in inducing hereditary variations. It was assumed that a measurement of the stimulus actually penetrating to the gonads would throw much light on the question under investigation, even if it did not supply a complete solution thereof. Congdon determined the penetration coefficient of heat, in the case of warm-blooded animals, by taking the rectal temperatures of rats and mice subjected to various temperature changes.[1]

Some of the more significant results of these experiments may be cited. Adult rats reared at 33° C. had a rectal temperature 1° higher than those reared at 16°, while rats and mice raised in either temperature showed no difference from the normal rectal temperature when approaching puberty. But they showed a change of 1° in rectal temperature if transferred from the higher to the lower temperature at that period. Adult rats and mice transferred from various temperatures to others differing by nine to seventeen degrees showed changes in rectal temperature ranging from 1.5° to 2°. In some doubtful cases, a change of 3° in the rectal temperature was recorded. *Myoxus glis,* a hibernating animal, showed a rise of 0.8° in rectal temperature on being transferred from a 14° to a 25° room. Congdon believes it probable that "testes fluctuate as much if not more in temperature than does the rectum" on account of their exposed position; but thinks that the ovaries and foetus being "more internally situated than testes would be expected to be more constant in temperature than the rectum." [2]

It is to be observed, in considering these facts, that all the body parts of warm-blooded animals exhibit a similar regulatory capacity with respect to temperature changes, so that if temperature changes can *directly* induce modifications of somatic tissues, there is no reason to suppose that they cannot induce similar modifications of the germ cells. But we are almost forced to the conclusion, in view of this very regulatory capacity, that changes in both cases come by way of, or as products of, the physiological adaptations to temperature changes which are

[1] "The Surroundings of the Germ Plasm. III. The Internal Temperature of Warm-blooded Animals (Mus decumanus, M. musculus, Myoxus glis) in Artificial Climates," *Arch. f. Entwm.*, Vol. XXXIII, 1912, pp. 703–715.

[2] *Op. cit.*, p. 712.

peculiar to warm-blooded animals. The changes in body weight, in length of the free peripheral parts, and in weight and hairiness of the pelt, as recorded in Sumner's and Przibram's experiments, could scarcely be attributed to any direct (or immediate) action of the temperature stimulus. There is certainly a long chain of physiological processes between the stimulus at the one end and the induced modifications at the other. Parallel considerations would probably apply to variations in the germ cells determining similar modifications in later generations. Although such considerations may seem to strain the hypothesis of direct action almost to the breaking point, the applicability of the hypothesis in its present form is not absolutely excluded thereby. For, as we shall see shortly, a later experiment has shown that the reproductive organs are extremely sensitive to environmental changes, and this discovery serves to lessen the strain put upon the hypothesis by the facts just cited.

Šećerov has undertaken, by means of ingeniously contrived experiments, to determine the penetration coefficient of light in the case of the black-and-yellow salamander.[1] These experiments showed that $^{172}/_{173}$ of the light falling on the body of the salamander is absorbed by the epidermis, and that therefore only $^{1}/_{173}$, or less than one per cent, penetrates into the underlying parts. It was estimated, however, that three or four times this amount penetrates the yellow spots of this animal.

In a parallel series of experiments conducted by the same investigator, the penetration coefficient for lizards was found to be very much lower, being only $^{1}/_{4500}$ in the region of the abdomen, and $^{1}/_{202500}$ in the region of the unpigmented posterior zone.[2]

Semon has argued from the low penetration coefficients for light and temperature stimuli thus demonstrated, that the hypothesis of parallel induction is untenable when applied to hereditary variations induced by these stimuli.[3] He points out that the hypothesis of direct action on the germ cells, as applied to hereditary variations in the size of yellow spots in the salamander, involves the assumption that the germ cells are incomparably more sensitive to light than the retina. For the latter was shown to be the stimulus receptor for the somatically induced enlargement of yellow spots in that animal, whereas light penetrating to the germ cells of the animal is only $^{1}/_{173}$ as intense as the

[1] "Die Umwelt des Keimplasmas. II. Der Lichtgenusz im Salamandra-Körper," *Arch. f. Entwm.*, Vol. XXXIII, 1912, pp. 682–702.

[2] Šećerov, Slavko, "Die Umwelt des Keimplasmas. IV. Der Lichtgenusz im Lacerta-Körper," *Arch. f. Entwm.*, Vol. XXXIV, 1912, pp. 742–748.

[3] *Op. cit.*, Chap. IX.

light falling on the retina; and Kammerer found that if the intensity of the light was much reduced in his experiments, little or no modification of the color pattern was produced.

But Stieve has recently shown, from experiments on Tritons, that the gonads are extraordinarily sensitive to slight changes in environmental conditions, and concludes from this fact that the hypothesis of parallel induction has not been undermined by the argument from the low penetration coefficients of external stimuli.[1]

In concluding our preliminary analysis of the hypothesis of parallel induction, some general observations on the situation just sketched may be in order.

As aforesaid, a complete impasse between the parallel inductionists and their opponents has apparently been reached. The former are now attempting to maintain their position against the argument from the low penetration coefficients of external stimuli. If they succeed in so doing, they will have done no more than prove the *possibility* of external stimuli inducing changes in the germ cells without the soma playing other than an instrumental rôle therein. They will *not* have proved that the rôle of the soma in this process is *only* of an instrumental character. Their hypothesis will still be unverified. On the other hand, should the somatic inductionists make good their argument from low penetration coefficients, they will not have verified their own hypothesis nor even disproved the opposing hypothesis. For advocates of the latter can fall back, at least for the more superficial characters, on the auxiliary hypothesis advanced by Weismann, and accepted by Roux and many others, that the somatic tissues contain germinal determinants corresponding respectively to the determinants assumed to be operative in the development of those tissues. This position would be a logically weaker one, with reference to the question under consideration, than that which assumes all germinal determinants to be located in the germ cells, for it would make the hypothesis of parallel induction dependent on two unverified hypotheses—(1) that germinal determinants are located in somatic tissues, and (2) that environmental stimuli act "directly" on these determinants—rather than on one such hypothesis, namely, that environmental stimuli act directly on the germ cells.

Moreover, neither of these hypotheses can be refuted or verified through the use of present methods. We know and may for a very long time know too little of the physiological processes in the body and

[1] "Über den Einflusz der Umwelt auf die Eierstöcke der Tritonen. Ein Beitrag zur Frage nach der Vererbarkeit erworbener Eigenschaften und der Parallelinduktion," *Arch. f. Entwm.,* Vol. XLIX, 1921, pp. 179–267.

in the germ cells, and of the manifold relationships between the two, not to mention our limited knowledge of the processes whereby the soma develops from the fertilized egg, and, particularly, what becomes of the genes or germinal factors in the process of development, to say what happens in the organism between the application of an external stimulus to it and the appearance of somatic modifications in the given organism and in its offspring. At the most we have not sufficient knowledge upon which to base a verifiable hypothesis respecting the genesis of hereditary variations induced by external stimuli.

This situation is due in the main to a faulty procedure in the formulation of hypotheses respecting this question, and the consequent difficulty, if not impossibility, of testing these hypotheses. This procedure is based on the assumption that the organism can be virtually divided up into its constituent factors and processes, and an *independent* causal efficacy (or lack of it) assigned to each group of factors or processes distinguished. Distinctions originally introduced for purposes of analysis are unconsciously converted into categories of independent entities, and hypotheses emphasizing one or another category of these imaginary entities appear, only to be opposed by hypotheses emphasizing other categories of imaginary entities.

While many of these hypotheses have had great heuristic value, they could not be conclusively refuted or verified. This is because each hypothesis has been based on indisputable facts, but failed to account sufficiently for the facts appealed to by rival hypotheses. Verifications and refutations in details have been possible, and these will doubtless be multiplied. But so long as we accept the unsound assumption of an organism divisible into parts, factors or processes, we cannot formulate anything approaching a valid theory of the origin of variations. We shall have to content ourselves with a superficial eclecticism which passively accepts the decisive verifications and refutations (in details) of the several hypotheses, until that assumption is abandoned, together with the highly artificial problems based on it. Yet a valid theory of variations should be possible, in general terms at least. Such a theory would represent, on its destructive side, the refutation of the particularistic theories now prevalent; and, on the constructive side, the synthesis of truths to be found in all those theories, and, more important, the formulation of workable problems for the future.

The Non-Representative Transmission Hypothesis

We have completed our preliminary analysis of the first two types of anti-Lamarckian theories which are to be subjected to a critical ex-

amination. The third type will not detain us here, as it has not commanded any serious support, and exists, indeed, only as a *possible* type of anti-Lamarckism. We refer, of course, to the possibility that functional activity plays a creative rôle in phylogenesis, but not one of a representative character. This possibility we shall take seriously, as our plan is to examine critically all the possible types of anti-Lamarckian theory, thereby preparing the way for such a merger of the Lamarckian principle into a synthetic theory of variations as may be sanctioned by our analysis.

Recapitulation

Let us recapitulate, in the light of our preliminary analysis, the several types of anti-Lamarckian theory which we are to examine.

There is the hypothesis that hereditary variations arise in the germ-plasm, without any representative contributions of the soma thereto, the function of the latter being to provide the germ-plasm with nutritive substances, oxygen and other necessary conditions of vital activity.

Two subsidiary hypotheses respecting the genesis of variations in the germ-plasm are distinguished: (a) One is the quasi-vitalistic hypothesis of Weismann himself, which attributes variations to vital activities on the part of the constituent elements of the germ-plasm, such as struggle for food or position, etc. This subsidiary hypothesis is not explicitly vitalistic in character, nor is it explicitly chemical. Its advocates have not been especially concerned to analyze the processes involved into their existential elements, but operate instead with empirical biological concepts. According to the later formulations of the theory, amphimixis does not play a strictly creative rôle in the genesis of variations, but only prepares the variations generated, through combinations thereof, for the action of natural selection. We need not, therefore, consider amphimixis in the analysis to follow. Since this form of the hypothesis is not incompatible with the view that variations may be qualitative as well as quantitative in character, we shall not insist much on Weismann's own view that all variations are of a quantitative character. The hypothesis is greatly strengthened by this concession respecting the qualitative nature of many hereditary variations.

(b) The other hypothesis interprets the genesis of variations in strictly chemical terms. It holds that all genes or factors are some sort of chemical bodies, and that the appearance of new, and the disappearance or modification of old, factors are due to chemical action. A

given variation might be due, on this hypothesis, to a change in the chemical character of one or more of the substances constituting the germ-plasm, or to an increase or decrease in the amounts of one or more of these substances, or to specific changes of both types combined. As this subsidiary hypothesis is discussed at length in connection with the physicochemical conception of life, it will be considered in relation to the Lamarckian principle only insofar as it supplies a contribution to anti-Lamarckian theory distinct from that of hypothesis (a).

It will be assumed in the analysis that every hereditary structure and function of the organism is represented by one or more factors in the germ-plasm, and that single factors may be operative in the ontogenesis of two or more hereditary structures or functions. It will be assumed, further, that these constituent factors are coördinated in a germinal organization of some sort, though it will not be necessary for our purposes to consider the nature of that organization. The question must be raised later on, however, whether the chemical theory of germinal variations can account for any sort of organization in the germ-plasm.

The question whether or no germ-plasm is located in body tissues as well as in germ cells is not a specially significant one for the analysis, but both suppositions will be duly considered so far as they bear on the analysis. We shall follow the assumptions of the several hypotheses themselves respecting the nature of the genes or factors constituting the germ-plasm, in order fairly to test the validity of those assumptions and of the hypotheses associated with them.

As previously stated, the anti-Lamarckian types of vitalism contribute nothing new in principle to anti-Lamarckism as such, and, for that reason, their consideration will be postponed to later chapters altogether.

The hypothesis of parallel induction, the second type of anti-Lamarckian theory to be considered, is closely associated with the germinal hypothesis, and so far as the latter must be considered in the examination of the former, we shall follow the assumptions and allow for the alternatives recognized in the preceding statement of that hypothesis.

In order to make our analyses of anti-Lamarckian theories as exhaustive as may be, we shall extend the category of environmental stimuli which might be conceived to induce hereditary variations through direct action on the germ-plasm, and assume that it may include not only stimuli of a more general character, such as changes in food, light, temperature, atmospheric pressure, chemical constitution of the medium, etc., but also more specific and localized stimuli, such as

one tissue might present to a neighboring or closely correlated tissue. This expansion of the category of environmental stimuli conceived as acting directly on the germ-plasm is not proposed in order to refute that hypothesis by testing its applicability to cases not interpreted on the hypothesis by its advocates, for that would of course be unjustifiable; but rather to test fully the explanatory possibilities of the hypothesis in relation to a large group of hereditary characters where it might be conceived to apply, as over against the Lamarckian hypothesis.

The application of all our anti-Lamarckian hypotheses is extended to a *comprehensive* group of characters, which we believe will furnish a conclusive test on the one side, of these hypotheses, taken separately and collectively, and, on the other side, of the Lamarckian hypothesis itself. We refer to what may be termed hereditary active adaptations of a specific character. This category of hereditary characters, as employed in this inquiry, will be defined in due course. Such an extended application of the anti-Lamarckian hypotheses is necessary to an exhaustive examination of anti-Lamarckian possibilities, although in the case of the parallel induction hypothesis and what we shall term, for short, the non-representative transmission hypothesis, this application is much more extended than has ever been proposed for these hypotheses. This, again, will not be deemed unfair to those hypotheses, once our purpose is understood.

Finally, let us remind ourselves that both the germinal and the parallel induction hypotheses are combined by anti-Lamarckians into a more or less unified theory as to the origin of variations. In order fully to test the possibilities of this combination, the two hypotheses are applied separately and collectively to the active adaptations employed as tests of these hypotheses. The non-representative hypothesis is also applied separately and in combination with other anti-Lamarckian hypotheses to this group of characters.

CHAPTER III

LAMARCKIAN AND ANTI-LAMARCKIAN INTERPRETATIONS OF ADAPTATIONS TO THE ENVIRONMENT

Abstract of the Argument

A brief abstract of the analysis to follow will perhaps aid in the comprehension and appraisal thereof.

Our contention will be, first, that there is a class of hereditary characters coterminous with life itself, which, on a critical analysis, can be accounted for only on the hypothesis that functional activity has exerted a representative influence in the genesis of those characters. The class of characters referred to includes, as aforesaid, *all* hereditary active adaptations to environment, which are of a specific character. It embraces all active adaptations of the organism as a whole to specific features of the environment, including purely inorganic features, and other organisms, whether members of the species under consideration or of other species. Active adaptations to other organisms usually have reference, it must be noted, to specific features, activities or behaviors of those organisms. The hereditary characters considered embrace, in addition, specific active adaptations between structures and functions of the same organism, including adaptations between constituent structures and functions of the cell, the tissue, the organ, the organ-system, and the organism as a whole. This group of characters may be designated as intra-organic adaptations. It will be convenient to consider in a separate chapter Lamarckian and anti-Lamarkian hypotheses in relation to this group of characters.

The crucial part of the argument is this: Hereditary active adaptations *must be defined, in part, in terms of those features of the environment* to which they are the adaptations. This means that both the organisms possessing such adaptations *and also their germ-plasms* must be defined, in part, in terms of those specific features of the environment. It is contended that, since the organism and its germ-plasm have specific hereditary relationships with a large number of specific environmental features, any hypothesis which assumes that functional activity in connection with those specific environmental features has not played a representative rôle in the genesis of the active

adaptations to them, must admit, when pressed to do so, that the genesis of the germinal variations determining the adaptations was harmonized with the specific features of the environment by some extra-organic agency, or at least by some agency of which no account can be given. We shall attempt to show that variations in the germ-plasm must, on the various anti-Lamarckian hypotheses under consideration, be chance variations, so far as any adaptive reference to specific features of the environment is concerned; and that there is little or no probability that such variations should determine active adaptations to the environment. This argument will of course be developed in connection with the several anti-Lamarckian hypotheses to be examined, and duly qualified for each of them.

This destructive part of the analysis will prepare the way for the incorporation of the Lamarckian principle, conceived as of general application, into a synthetic theory of variations. To avoid misunderstanding, it must be repeated that an exclusive causal rôle will not be claimed for functional activity, in the genesis of hereditary active adaptations, for, as we have insisted, physicochemical and organizatory factors are always associated with functional activity in all vital processes, including all phylogenetic processes.

Environmental factors themselves are always operative in phylogenesis, if our analysis be correct, since active adaptations are adjustments to specific features of the environment and cannot be accounted for without assuming that the latter play a causal rôle in the process. Moreover, active adaptations and the environmental features correlated therewith are always associated with, when they are not wholly constituted by, physicochemical factors, and these factors cannot be conceived as wholly inactive in the process of generating those adaptations. Further, the cells, tissues and organs of the body have all their *hereditary* physicochemical characters, and their activities must be conceived, in part, in terms of physicochemical processes. It is certain, therefore, that features of the environment corresponding to specific active adaptations, together with physicochemical substances and processes associated with and in part constitutive of the structures and functions involved, are universally operative in the genesis of active adaptations.

Moreover, as we shall endeavor to show in later chapters, organizatory factors are also operative in all distinctly vital processes, including all phylogenetic processes. In none of this, be it remembered, do we assume the real separability of functional, physicochemical and morphogenetic factors, so far as the organism is concerned. The organism is not possible or conceivable if any group of these factors

be wanting. We contend, however, that the organism and its activities must be considered from the viewpoint of each group of factors or properties, if anything like a valid account of phylogenesis is to be given. But since all the matter and energy combined in living organisms once existed and could exist again in an inorganic state, we shall need to consider possible ontological (or pre-organic) distinctions in the substances or factors combined in living organisms. And if non-material factors are operative in vital phenomena, possible ontological distinctions among the several factors of this category should be considered. It should be possible, through a supplementary analysis of this sort, to provide a metaphysical foundation for our synthetic theory of variations, though it be only of a provisional nature.

Finally, it will not be necessary to claim that variations do not originate in the germ-plasm, whether by chemical or non-chemical action, or that environmental changes do not induce variations in the germ-plasm without the intervention of the soma, or, again, that functional activity does not play a non-representative causal rôle in the genesis of hereditary variations. It would be hazardous to make any such claims, for we have evidence that some hereditary variations at least must be attributed to factors falling within one or another of these categories. Nor need we claim that such factors are not involved in the genesis of active adaptations. It may be that such factors are involved in that process, while it is certain that physicochemical and organizatory factors (taken in a general sense) are involved therein. Our contention is that the genesis of specific active adaptations cannot be adequately accounted for by any possible combination of such factors, but that functional activity must be allowed to play a representative rôle therein.

Objections to our methods and to the results attained thereby, whether such objections be based on logical or empirical grounds, will be considered after the positive part of the analysis has been completed.

Some further explication of the terms employed in our analysis, and we shall be ready for the examination of cases. The term "specific active adaptation" is taken to designate specific *activities,* rather than specific *structures,* although such activities are often associated with more or less specific structures. It is important to keep this distinction in mind, as certain adaptations difficult to explain on any hypothesis are rendered more intelligible by the supposition that the structures involved have experienced a substitution of functions at some stage of their evolutionary history. The wide differences in the functions of homologous structures illustrate the frequency and im-

portance of such functional substitutions. In making the distinction between structures and functions, we do not imply that they are separable in reality. Indeed every structure has its specific functional activity or activities, which may or may not be strictly determined by heredity, while the substitution of one function for another, in the case of a given structure, is very likely preceded, accompanied or followed by some modification of the structure itself. In our analysis of intra-organic adaptations we shall sometimes find it convenient to use structure and function as interchangeable terms. In this case, however, we shall not be departing from our definitions, but merely taking structure as representative of the associated function, or functions.

Specific active adaptations, in the sense employed here, are to be distinguished from active adaptations which do not have reference to specific features of the environment. Many active adaptations have reference to broad categories of environmental stimuli, which differ much among themselves. That is true, for example, of food adaptations in the case of many species, and, for the human and probably some other species, of adaptations to cultural factors of the environment. Active adaptations of this type are not different in kind from the more specific adaptations, and would necessarily be interpreted on the same principles. But we shall attain more decisive results if we select, as tests of Lamarckian and anti-Lamarckian hypotheses, active adaptations of the more specific type. This will not represent, however, a selection of evidence more favorable to any of the rival hypotheses than to the others. Objections which might be drawn from this selection of evidence will, nevertheless, be duly considered.

Active adaptions themselves are of course distinguished from passive adaptations and from characters having no demonstrable adaptive value. Passive adaptations are serviceable from their mere presence, and do not represent genuine functional activity on the part of the organism. Protective body coverings, mimicry, spines, stings and poisons are typical adaptations of this class. Characters with no demonstrable adaptive value are illustrated by the detail of color patterns, the peculiarities of feather-venation and the like. We can distinguish, in addition, a class of hereditary characters which may be designated as non-vital adaptations. Certain of the tropisms might be taken as illustrating this class of characters.

The bearing of these several groups of characters on our analysis of active adaptations must needs be considered, however, especially insofar as objections to our conclusions might be drawn from them.

The term functional activity is employed throughout our inquiry, in a broad sense, and may refer to any type of activity characteristic of

the organism as a whole or of its constituent parts. This usage avoids the connotations of the old term "use and disuse," and the inconveniences consequent thereon. Functional activity in the sense here employed does not imply "effort or willing," or mental processes of any sort, although such processes are certainly involved in a great deal of functional activity. The term, though taken in this broad and neutral sense, is sufficiently specialized in its application to the specific adaptations analyzed. When we speak of functional activity of a certain kind as having reference to specific features of the environment, we do not mean any conscious reference, though the latter may be assumed in particular cases. The inner nature of that reference, if we may so speak, is not brought into consideration.

Before taking up our analysis of specific adaptations, reference may be made to some of the more basic adaptations of the organism to its environment. "It is a truism," says Cope, "that change of physical conditions has preceded all great faunal changes, and that the necessity for new mechanism on the part of animals has always preceded the appearance of new structure in geologic times." [1] The experience of plant breeders has amply demonstrated a similar dependence of floral changes on changes in environmental conditions.[2] Not only is there a close correlation between phyletic and environmental changes, but all the characteristics of the organism have explicit reference to definite features of the environment. "In every case," says L. J. Henderson, "the particular characteristics of the organism fit a special environment, while [reversing the point of view] the general physical and chemical properties of water and carbonic acid fit the general characteristics of life." [3]

These facts are of course readily conceded by all parties to the controversy respecting the genesis of variations; it is the interpretations put upon the facts that differ so radically.

Adaptations to the Medium

Our first group of cases is drawn from adaptations to the physical environment, and, more particularly, to media inhabited by various groups of organisms. The media of animal organisms may be classified as follows: (1) land (geobios), subdivided into (a) media with light (diaphanic) and (b) other media without light (aphanic or sub-

[1] *The Origin of the Fittest,* p. 351.

[2] Cf. Kellogg, Vernon L., *Pop. Sci., Mo.,* Vol. LXIX, 1906, pp. 363–374, where the important rôle of new, and particularly of more favorable, environmental conditions in the breeding experiments of Luther Burbank is clearly brought out.

[3] *The Order of Nature,* p. 6.

terranean); (2) saltwater (halobios), subdivided into (a) diaphanic and (b) aphanic (or abyssal), the diaphanic itself being subdivided into planktonic and littoral; (3) freshwater (limnobios), subdivided into (a) diaphanic and (b) aphanic, with the diaphanic subdivided into planktonic and littoral, and the aphanic into abyssal and subterranean; (4) combinations of two of the specific media distinguished (diplobios), including (a) land and freshwater, (b) land and saltwater, and (c) freshwater and saltwater; (5) media (entobios) constituted of plant or animal organisms (acting as hosts to parasites), plus an aquatic or terrestrial medium in which the parasites have their free stages. Diplobois media could be further subdivided, according as the organisms adapted thereto dwell in the constituent media alternatively throughout life, or have different stages of their development in the two. Entobios media likewise could be subdivided according to the major types of parasitism distinguishable, as ento- or ectoparasitism, the ontogenetic position of the free stage, the medium of the free stage, the taxonomic groups to which the hosts belong, etc.

A point of special interest for a later part of the analysis is the fact that the medium of the entoparasite during its parasitic stage is a specific organic medium, most species of entoparasites being adapted to one or a few species only, and the great majority of them to only one species each.

Various groups of media may be still further specialized by taking into consideration differences in temperature, and the restriction of particular species of animals to particular ranges of temperature. Certain media are also correlated with differences in pressure, as in the case of animals living at various depths of the sea.[1]

More generalized adaptations to the medium may be referred to before undertaking our analysis of specific adaptations thereto. As Henderson has pointed out, "the general physical and chemical properties of water and carbonic acid fit the general characteristics of life." [2] Most animals and plants are also dependent on free oxygen, although the structures whereby they appropriate and utilize this substance are very diverse. All organisms are likewise dependent on food of one sort or another, this of course being necessary to growth, repair of tissue and, in the case of mobile organisms, the production of kinetic energy. Many species are hereditarily adapted to one or a few kinds of food only.

[1] This classification of media is adapted from Montgomery, T. H., *The Analysis of Racial Descent in Animals,* pp. 6–13.

[2] *Loc. cit.* The adaptability of the earth's surface to life as we know it has been demonstrated at length by the same author in *The Fitness of the Environment.*

The more fundamental of the characteristics noted, such as the adaptations of organisms to water, free oxygen and nutritive substances, were present at the very beginning of life, as we conceive it, and speculation concerning these characteristics falls, therefore, outside the scope of our inquiry. Such adaptations, though representing a high degree of solidarity between the organism and its environment, must have originated through some sort of interaction between primordial organizatory factors and the substances in question. We know little, if anything, regarding the nature of those interactions. All these generalized adaptations were of course established long before any germ-plasm was differentiated from other parts of the organism.

Hereditary adaptations to the medium have of course been modified since specialized reproductive systems were evolved. We can offer only some very general considerations regarding adaptations of this sort.

Let us consider first the modification of adaptations necessitated by changes of temperature. A range of temperature constituting the temperate medium of a given species may be considered a specific feature of the environment. The non-chemical form of the germinal hypothesis would seem to be ruled out as supplying an interpretation of such modifications, by some very simple logical considerations. An interpretation of such modifications on that hypothesis involves the assumption that variations of the germinal factors correlated with temperature adaptations are generally coincident with the successive ranges of temperature to which the organisms in question are subjected; or, instead, that germinal variations determining modified temperature adaptations occur without any reference to actual temperature changes.

On the first assumption, correlation between germinal variations and temperature changes affecting the organisms in question must be effected by some external agency, a consequence repugnant to the fundamental postulates of modern science; while on the second assumption, the germinal variations determining modified temperature adaptations would in a very large proportion of cases lead to such maladaptations with respect to temperature, as to result in the extinction of the organisms concerned. For, according to the germinal hypothesis thus interpreted and applied, variations in the germ-plasm might determine adaptations to any one of all the possible ranges of temperature, and without restriction to the minimal or maximal points beyond which life of any sort is impossible. If we let the letter n represent such possible ranges of temperature, then the chances that a given germinal variation will adapt the organism concerned to the range of temperature to which it will be subjected (whether the existing or some other

range of temperature) will be $1/n$. This process indefinitely continued would certainly lead to the depopulation of the earth's surface.

Variations in the germ-plasm interpreted chemically would be just as contingent with respect to temperature changes, and the same considerations would therefore apply to this interpretation of modified temperature adaptations as to that supplied by the non-chemical form of the hypothesis.

The hypothesis of parallel induction, when applied to this case, meets with difficulties of a comparable nature. It is certain that the system of functional activity peculiar to a given species is very closely bound up with its temperature medium, since if members of this species are subjected to a temperature overstepping the minimal or maximal point of that medium, their physiological activity is so deranged that death results. Historical geology and geographical distribution demonstrate, however, that species can often, if not generally, become adapted to lower or higher ranges of temperature than those to which they have been accustomed. But there are decided limitations, varying no doubt for different species, on the rate of temperature change to which organisms can progressively adapt themselves.[1]

Such gradual temperature readaptations are certainly to be identified with progressive modifications of a profound character, in the functional activity of the organisms concerned. And it is a safe inference that hereditary modifications in the system of functional activity induced by temperature changes have much in common with, if they are not quite equivalent to, modifications in the activity of organisms first subjected to the changes in question, for both the acquired and the hereditary modifications represent changes in functional activity adapted to the same conditions.

Now, on the hypothesis of parallel induction, we should have to assume that two *independent* systems—the germ-plasm and the soma—respond in such a way to the external stimuli in question that substantially equivalent results are produced. We are not here repeating the objections earlier set forth, against the assumed division of the organism into virtually independent systems, and the assumption of a virtu-

[1] For a remarkable experiment on the acclimatization of flagellate organisms (*Tetramitus rostratus, Monas Dallingeri* and *Dallingeria Drysdali*) to progressively higher temperatures, see Dallinger, W. H., "The President's Address," *Jour. Roy. Micr. Soc.*, 1887, pp. 185–199. These animals, which flourished at the beginning of the experiment in a temperature of 60°F., and were destroyed by a temperature of 78°, were acclimatized through a progressive increase in temperature extending over a period of seven years to a temperature of 158°. A summary of this experiment with a discussion of the possible physiological changes involved will be found in Jennings, H. S., *Life and Death, Heredity and Evolution, in Unicellular Organisms*, pp. 98–100.

ally perfect continuity and segregation of the germ-plasm. We are here concerned to exhibit the empirical consequences of those assumptions and of the hypothesis, here in question, which is based upon them. If we have, as we do have, on this hypothesis and these assumptions, two independent systems generally acting with reference to each other, and producing equivalent results, then that mutual reference and that equivalence in results call for explanation. And some extra-organic agency must evidently be invoked for this purpose. The position of the hypothesis would not be bettered, should it be assumed that germinal determinants are situated in somatic tissues, and that these communicate the changes induced directly by the external stimuli, to the corresponding determinants in the germ cells. For the fundamental assumptions of the hypothesis remain the same, and involve the same consequences. The determinants, wherever located, are conceived to be independent of the somatic tissues, and the interpretation of correlative changes in the two must ultimately rest, according to this conception, on the assumption of some external coördinating agency.

A brief consideration of the non-representative transmission hypothesis will suffice for this case. If, as we inferred, the hereditary modifications of functional activity induced by temperature changes are substantially equivalent to the acquired modifications of such activity in the organisms originally subjected to those changes, the interpretation of the hereditary modifications on the non-representative hypothesis would involve the assumption that specific modifications constituent of the whole group of functionally acquired modifications had not, by way of germinal modifications, determined the corresponding specific modifications of somatic activity in later generations, but had determined *other* specific modifications of somatic activity therein. Evidently, therefore, specifically different modifications in offspring produced by the modified functional activities of the parental organisms would need to be coördinated in the right way, so as to secure equivalent series of changes in the earlier and later generations. An external coördinating agency would need to be invoked if such coördinations were accounted for. The position of this hypothesis would not be bettered by the supposition that specific acquired modifications in the earlier generations could, by chance, determine equivalent hereditary modifications in later generations, for, considering the number and necessary ordering of the specific modifications involved, the chance of such an occurrence would be exceedingly small. We then have virtually the same result as before.

This leaves only the Lamarckian hypothesis as a possible explanation of such adaptations. This hypothesis involves none of the impossible

empirical consequences implied by the opposing hypotheses, and it is of course positively supported by the inferred equivalence of acquired and hereditary modifications in functional activity induced by temperature changes. Its only difficulty is to account for the postulated representative effect of functional activity on hereditary modifications. It is now generally conceded that objections drawn from this difficulty are by no means decisive, and facts are accumulating which strongly indicate the existence of certain processes whereby functional activity could have a representative effect on hereditary modifications. This question will be discussed in a later chapter, and need not be adverted to in our further consideration of active adaptations. Subject to our success in meeting objections to the type of analysis here set forth, drawn from its apparent inapplicability to certain types of active adaptations, as well as objections to the methods and assumptions of the analysis—objections which will be considered in a later place—, we may say that, if our argument be correct, modified temperature adaptations, regarded as active adaptations to specific features of the environment, must be interpreted in part on the Lamarckian principle. Physicochemical and organizatory factors are also involved, it is true, but these taken singly or in combination cannot wholly account for specific active adaptations to environment, as we shall hope to demonstrate in later chapters.

To avoid tedious repetition, objections based on an emphasis of the part played in phylogenesis by physicochemical and organizatory factors, on the alleged inconceivability of a transmissionist mechanism, and on a consideration of the general methods and assumptions of our own analysis, will not be rebutted concurrently with the analysis of further cases employed as tests of Lamarckian and anti-Lamarckian hypotheses, but will be rebutted in detail after the positive part of the analysis has been completed.

An analysis similar to that just presented would apply to anti-Lamarckian interpretations of modifications in adaptations induced by other changes in the medium, including change from light to darkness, land (and air) to freshwater or saltwater, and freshwater to saltwater, together with changes the reverse of these, as well as changes of various kinds in the diplobois media inhabited by many species, whether consisting in different combinations of primary media, or in the organism's ontogenetic relations to these media. Since the same analysis would apply, *mutatis mutandis,* to all these cases, we may save ourselves the tedium of repeating it in relation thereto. We should find that all the anti-Lamarckian hypotheses involved, in the last resort, the

postulation of some external coördinating agency in order to make the germinal variations fit the actual environmental changes, or, in the case of the non-representative transmission hypothesis, to make the several series of acquired and of hereditary modifications produced by functional activity equivalent. The hereditary adaptations of parasites to their peculiar media (the species serving as hosts) seem, however, to furnish such a decisive test of Lamarckian and anti-Lamarckian hypotheses, that typical instances of such adaptations will be analyzed at a later stage of the discussion.[1]

Adaptations to Other Species of Organisms

Let us next consider, in relation to our several hypotheses, a striking, though typical, hereditary food adaptation. We may reproduce the

[1] Before leaving the present topic, we must record our recognition of Cunningham's forceful argument for the Lamarckian interpretation of certain specific adaptations to the medium. This argument will be better set forth in Cunningham's own words: ". . . metamorphosis," he writes, "can only be explained on the principle that the different conditions acting on the individual at different periods of its life give rise to and determine the direction of the modifications which characterize the successive stages of the individual structure. . . . We can have no doubt that the air-breathing Amphibia were evolved from fishes, though we may not be able to say exactly what kind of fishes . . . how can we conceive the conversion of a single individual fish into an air-breathing creature, apart from the change of conditions, the breathing of air? . . . the structural arrangements connected with the action of lungs, cannot be conceived apart from the respiration of atmospheric air. We know of plenty of cases in which, the water being scarce or foul, fish have become capable of breathing air, in one way or another, but we have no evidence of the occurrence of variations in adult life tending towards air-breathing structures in fishes which are never exposed to the air. We do not find them, for instance, in fishes that live on the sea-bottom or in the ocean abysses." "The Species, the Sex, and the Individual," *Nat. Sci.,* Vol. XIII, 1898, p. 236.

We may also reproduce Cunningham's description and interpretation of a remarkable adaptation to a special type of diplobios medium—a combination of water and atmospheric air. ". . . there is a fish which has its eyes in a very remarkable condition. Spectacles for our own eyes, for human eyes, are sometimes made in which the upper half has a curvature different from that of the lower. The fish to which I refer, *Anableps,* does not wear spectacles, but actually has its eyes made in two parts, in the upper part of which the lens has a different curvature from that of the lower. The pupil is also divided into two by prolongations from the iris. This fish is in the habit of swimming at the surface with its eyes half out of the water, and the upper half of the eye is adapted for vision in air, the lower half for vision under water. Now, however various the individual variations in fishes' eyes, there is no evidence that variations which could by selection give rise to this curious condition, occur in other species of fish. It seems to me that we have no reason to suppose that the required variations ever occurred, until the ancestors of *Anableps* took to swimming with their eyes half out of the water. A similar argument applies to many other cases of special adaptation, and the logical conclusion is that the habits and conditions determined the modification." "The Species, the Sex, and the Individual," *Nat. Sci.,* Vol. XIII, 1898, p. 189.

vivid account of this case contained in a popular treatise by Jordan and Kellogg.[1]

"There was introduced into California from Australia, on young lemon trees, twenty-five years ago, an insect pest called the cottony cushion scale (*Icerya purchasi.*) This pest increased in numbers with extraordinary rapidity, and in ten years threatened to destroy completely the great orange orchards of California. Artificial remedies were of little avail. Finally, an entomologist was sent to Australia to find out if this scale insect had not some special natural enemy in its native country. It was found that in Australia a certain species of ladybird beetle attacked and fed on the cottony cushion scales and kept them in check. Some of these ladybirds (*Vedalia Cardinalis*[2]) were brought to California and released in a scale-infested orchard. The ladybirds, having a plenty of food, thrived and produced many young. Soon they were in such numbers that many of them could be distributed to other orchards. In two or three years the Vedalias had become so numerous and widely distributed that the cottony cushion scales began to diminish perceptibly, and soon the pest was nearly wiped out. But with the disappearance of the scales came also a disappearance of the ladybirds, and it was then discovered that the Vedalias fed only on cottony cushion scales and could not live where the scales were not. So now, in order to have a stock of Vedalias on hand in California, it is necessary to keep protected some colonies of the cottony cushion scale to serve as food."

It will no doubt be conceded that we have here a hereditary adaptation to a very specific sort of food, since when that species of food is wanting the organisms possessing the adaptation must perish. The adaptation, moreover, is to a unique, and what may be regarded as an irreproducible, feature of the environment, considering the fact that the cottony cushion scales were produced by a long sequence of complex causal factors which could scarcely be repeated. It should be possible to draw from such an adaptation a decisive test of rival hypotheses respecting the origin of hereditary variations. Let us see how those hypotheses meet the test supplied by this case.

The preferred anti-Lamarckian interpretation of this case would doubtless be drawn from some form of the germinal hypothesis, properly so-called. We may therefore consider at some length the interpretation of the case on this hypothesis. We shall deal first with the non-chemical form of the hypothesis. This form of the hypothesis

[1] *Evolution and Animal Life,* p. 64; D. Appleton and Company, publishers. See also Kellogg, V. L., *American Insects,* pp. 186–187, 287–288.

[2] Since renamed *Novius Cardinalis:* Note by present writer.

would imply that a germinal variation had occurred in the ancestors of the ladybirds which determined a modification of their food adaptations, including an exclusive food adaptation to the cottony cushion scales. Such a variation, be it noted, must have arisen without the cottony cushion scales being involved in the process. The assumed variation may therefore be termed a contingent one, considered with reference to the scale insects themselves. What are the chances that such a variation would occur?

According to careful estimates, about two million different species of insects are extant at the present time, though only a fraction of this number have been identified and their systematic positions determined.[1] The total number of animal species now living is of course much greater than that. These facts will give us some notion as to the number of animal species which existed in the world at the time the variation in question occurred. Now, supposing that the germ-plasm of the ladybird's ancestors *could* have varied in such a way as to bring them into some sort of adaptation to any one of the animal species contemporary with them, and that the probability of variation adapting those organisms to any one of those species was not greater than that of variation adapting them to any other one of the same species (the germinal hypothesis implies that this was the case), we must conclude that the chance of such a variation occurring as is here in question was exceedingly small. The probability of this variation occurring, on the assumptions of the germinal hypothesis, may be represented by a fraction, of which the numerator is one, and the denominator the number of the animal species existing at that time, doubtless several millions.

But an interpretation of the hypothesis even less favorable than the foregoing could be fully justified. For, so far as the hypothesis can tell us, the germ-plasm of the ladybird's ancestors could vary in such a way as to adapt it, in one way or another, to any one of the animal or plant species which have ever existed or which may hereafter exist, and there was no greater probability of a variation occurring which should adapt it to the cottony cushion scales, than of a variation adapting it to any other species, past, present or future. Moreover, the chance of a germinal variation adapting the ladybirds to any other species *as food* was no greater than the chance of a variation determining a specific adaptation of a different kind. This probability, when duly allowed for, multiplies by some indeterminate number the denominator of our fraction representing the probability of the variation in question occurring according to the process postulated by the germ-

[1] Kellogg, V. L., *Darwinism To-day*, p. 22.

inal hypothesis. The hypothesis must also account for the spatial and temporal coincidences between the actual modification of the ladybird's food adaptations and the distribution of the cottony cushion scales. Mathematically considered, however, the factor for such coincidences is included in the formula allowing for the number of species (past, present and future) to which adaptations were possible in principle, and for the different kinds of active adaptations to such species possible on the same principle. If we let m represent the number of all possible species, and n the possible different kinds of active adaptations to other species, the chance of a variation determining an exclusive food adaptation of *Novius Cardinalis* to *Icerya purchasi* may be expressed by the fraction, $1/mn$. No exact value for this fraction can of course be computed, but we are justified in affirming that it is infinitesimally small.

Some further considerations respecting this case will be in order. The cottony cushion scales constitute, as aforesaid, a unique and irreproducible feature of the (ladybird's) environment. They are the resultant of an enormous number of causes, arranged in a particular historical sequence. This *sequence* of causes could not possibly be repeated, and many, if not most, of the causes themselves had no equivalents at the time the ladybird's adaptation to the scale insects was generated, since some of those causes at least were other species of organisms which had long been extinct or succeeded by descendant members of their phyletic series. This unique and irreproducible feature of the environment, therefore, was and is radically heterogeneous with everything else in the world, considered with respect to its *specific characters*. It has of course many things in common with other species of organisms, and especially with the species most nearly related to it.

Now, such a feature of the environment could not be represented in respect to its uniqueness by anything else in the world, without some sort of prior reaction to or acquaintance with this feature by the thing representing it. There is in the ladybird's hereditary food adaptation a genuine representation (special in kind) of the cottony cushion scales, since the adaptation is to this particular and unique species of scale insect. It would be a miracle if such representation had arisen without some prior interaction or acquaintance with the unique thing represented.

We may say, on the basis of these considerations, that it is a logical absurdity to suppose that a variation could arise through the process postulated by the germinal hypothesis, which should determine a specific adaptation of the organism possessing it to the members of another species. Could we assume, on the most favorable view of the

hypothesis, that such a variation might occur, it could *not* occur *often enough* to account for more than a very small portion—an infinitesimally small portion, we should say—of the hereditary active adaptations to other species which we find in nature.

The assumption of a regulative selective process, which is of course an integral part of the germinal theory, carries destructive consequences for that theory, when brought into connection with applications of this sort. A germ-plasm that had to produce millions of useless variations for every one that happened to fit some specific feature of the environment would itself be so unfit that it would be promptly eliminated by the selective action assumed to be operative in phylogenesis. But, since more or less specific adaptations to environment, including adaptations to other species, are indispensable to different species of organisms, and since all germ-plasms must, on the germinal hypothesis, vary indiscriminately with respect to the specific features of the environment, including other species, every species must, according to the implications of the hypothesis, be eliminated, because of the incapacity of its germ-plasm to provide it with the necessary specific adaptations to the environment. The germinal hypothesis thus destroys itself, when its implications are laid bare.

We are indebted to H. S. Jennings for the suggestion of a process apparently not susceptible of a Lamarckian interpretation, whereby this particular adaptation might have arisen. It is introduced at this point because it will serve both to test and to illustrate certain of the preceding arguments. Jennings writes: "Taking your example of the beetles that prey only upon the particular species of scale insect, it is possible, and indeed seems most probable that earlier it preyed upon various insects having some common features and that later some change occurred by which it was restricted to this one. But this change may perfectly well have been, so far as I can see, some alteration in the form of the jaws or other part of the prehensile or the digestive apparatus, such as excluded preying upon others." [1]

Regarding this suggestion, we may remark, first, that there is no evidence that the modification in question was of the sort indicated. Furthermore, it is extremely improbable that a change in the prehensile or digestive apparatus of the ladybird could have restricted it to a certain species of scale insect, since the specific differences distinguishing these species from one another could not *all* have been such as to render them (with one exception) unavailable as food for the ladybirds, on account of the hypothetical change in the mandibular or digestive apparatus of the latter. Finally, if a hereditary modification so special

[1] From a private letter to the writer.

in character did occur, it was an instance of a variation adapting the organism possessing it to a single one of the several million species existing in the world. The interpretation of such a variation on the germinal hypothesis would be open to the criticism we have directed against the application of that hypothesis to just this case, but without a special interpretation being annexed thereto. Jennings has only offered us a hypothetical construction of the adaptation in question. Even were his construction the correct one, it would remain a hereditary adaptation of one species to another species.

The interpretation of this case on the chemical form of the germinal hypothesis is subject to the identical criticism directed against the interpretation thereof on the non-chemical form of the hypothesis, since both forms of the hypothesis assume that the modification in question occurred without the cottony cushion scales being involved in the process. The chemical form of the hypothesis gets into difficulties special to itself when it tries to account for the *organization* represented by the ladybird's food adaptation and its modification, since such organization is *assumed* by the non-chemical form of the hypothesis, but cannot be assumed, without an account first being given of it, by the chemical form of the hypothesis. These special difficulties of the latter hypothesis will be considered in a later place.

It is conceivable that the chemical compounds in the germ cells of the ladybird's ancestors were of such a nature as to make the assumed accidental variations therein come nearer adapting them to some species of organism than to others, in which case the chemical form of the hypothesis would have an advantage over the non-chemical form. We cannot of course consider such a possibility unless the grounds thereof should be stated in such a way that we might deal with it. The hypothetical advantage carried by this possibility, however, would seem to be more than offset by the difficulties which confront the hypothesis in its attempt to interpret organization of the sort represented by this adaptation, in terms of chemical reactions. In any case chemically produced variations in the germ-plasm would be highly contingent, considered with reference to another species of organism, and could in no wise represent such a species, as does the adaptation we are considering.

The interpretation of this case on the hypothesis of parallel induction meets with difficulties of a different sort. As in the interpretation of adaptations to the medium on the same hypothesis, we should have to assume that the parallel series of effects produced in soma and germ cells by the stimulus of the cottony cushion scales were coördinated by some external agency, in order to account for the equivalence of the acquired and hereditary food adaptations. Any consideration of the hy-

pothesis in relation to this case must of course rest on the supposition that the cottony cushion scales could as stimuli act directly on the ladybird's germ-plasm. But no such supposition could be seriously entertained, since visual, auditory, olfactory or other stimuli emanating from these insects could hardly be supposed capable of inducing a modification of any sort in the ladybird's germ-plasm. Were environmental stimuli of this sort credited with the capacity of inducing modifications of the kind under consideration, we should be endowing them with a creative power of a truly miraculous sort. For one could not explain why a food adaptation rather than some other sort should be modified, or why visual or other stimuli from the cottony cushion scales should induce a hereditary modification of the ladybird rather than of other species in the same area, or why the cottony cushion scales should be more effective than other species of the same area in modifying the ladybird's germ-plasm. Since, as we have said, this case would probably not be interpreted by anti-Lamarckians on the hypothesis of parallel induction, we need not consider further this possible application of the hypothesis.

Nor need the non-representative transmission hypothesis long detain us in relation to this case. Supposing functional activity to be a factor in the genesis of the adaptation in question, we could not imagine any sort of functional activity leading to that adaptation, except that which the adaptation itself later determines. If we could conceive action of this sort, and regarded it as a typical phylogenetic process, we should have to postulate an external coördinating agency in order to account for the equivalent systems of functional activity in the generations evolving the hereditary character, and in the generations endowed with the hereditary character thus evolved.

The only hypothesis remaining is that which ascribes to functional activity a representative rôle in the genesis of active adaptations. This hypothesis is supported *negatively* by the *exclusion* of all other logical possibilities, and *positively* by the *converse* of the arguments opposed to the interpretation of the case on the germinal hypothesis. Those arguments purport to demonstrate the impossibility of one species becoming hereditarily adapted to another species without prior reaction to or acquaintance with that species. The Lamarckian hypothesis finds its primary support, however, in the inferential evidence drawn from a consideration of the influence of functional activity on individual development. That evidence is reinstated by our examination of hypotheses denying a similar influence of functional activity on hereditary structure. If there have been no lacunae in our analysis, therefore, the conclusions drawn from it must be accepted, unless some other

possibility of interpreting this case in an anti-Lamarckian sense can be demonstrated.

In the foregoing analysis, the adaptation of *Novius Cardinalis* to *Icerya purchasi* has been taken as typifying hereditary adaptations of one species to another, and the validity of our conclusions does not depend, therefore, on the facts of this particular case. Our analysis applies to every hereditary adaptation of one species to another, and, *mutatis mutandis,* to all hereditary adaptations to other specific features of the environment. We believe the truth of this claim is conditional only on our success in meeting objections drawn from specific adaptations deemed refractory to the analysis here set forth, as well as objections to the methods and basic assumptions of the analysis. As we have already promised, such objections will be dealt with in their proper place.

Adaptations to Spatial Relationships

Let us take as our next case the chick's hereditary adaptation to the relationships of three-dimensional space. To get the salient facts before us we may cite observations by various students of the chick's behavior. We reproduce from Lloyd Morgan, Spalding's account of a young chick's behavior, when first introduced to the world of visual objects:

Spalding describes how a chick, which had been blindfolded at birth, was placed, twenty minutes after it had been unhooded, "on rough ground, within sight and call of a hen with a brood of its own age. After standing chirping for about a minute, it started off towards the hen, displaying as keen a perception of the qualities of the outer world as it was ever likely to possess in after-life. It never required to knock its head against a stone to discover that there was 'no road that way.' It leaped over the smaller obstacles that lay in its path, and round the larger, reaching the mother in as nearly straight a line as the nature of the ground would permit. This, let it be remembered, was the first time it had ever walked by sight." [1]

Observations of the chick's pecking reactions further support the view that the chick is endowed at birth with a capacity for accurately estimating distances by means of visual stimuli, and for reacting more or less adaptively to these stimuli by virtue of such capacity. Spalding himself overestimated the accuracy of the first pecking reactions, as

[1] Morgan, Lloyd, *Habit and Instinct,* p. 34. Quotation is from Spalding, D. A., "Instinct. With Original Observations on Young Animals," *Macmillan's Magazine,* February, 1873, Vol. XXVII, p. 289.

Preyer,[1] Morgan,[2] Breed[3] and others have shown. But all observers agree that, while the young chick often misses the objects pecked at, it always comes very close to them, except in rare cases where the pecking reaction is interfered with.[4] "Most noteworthy is the fact," says Lloyd Morgan, "that the young birds only strike at objects which are well within striking distance; they have not to learn this distance by experience. To peck at an object of a certain size, just within easy reach, is a definitely congenital response, and not the result of acquired skill."[5]

Breed's observation of a certain chick, when on the experimenter's table, supports the same view: "The animal could easily be pushed away from the edge but, when near the edge, *resisted strongly* if pushed toward it. This same behavior was noticed in other chicks. For example, no. 14, the day after it was hatched, when on the experiment table for the first time, was pushed toward the edge. It resisted by bracing its legs in front of itself and hurried back from the edge as soon as it was released."[6]

Even more delicate discriminations between distances, on the chick's part, are revealed by Thorndike's investigations. He tried the experiment of putting young chicks (95 hours old) on boxes of various heights and observing their reactions in these situations.[7] When placed on boxes less than ten inches high the chicks jumped down at once; placed on a box sixteen inches high, there was some hesitation before jumping down, and the delay in the jumping-down reaction increased correlatively with the height of the box on which the chicks were placed, until at a height of thirty-nine inches the chicks would not jump down at all. The same experimenter observed that the chick does not peck at objects remote from him.[8] Thorndike concludes from these and other observations that "in the ways he moves, the directions he takes and the objects he reacts to, the chicken has prior to experience the power of appropriate reaction to colors and facts of all three dimensions."[9]

Let us consider the interpretation, on our several hypotheses, of the

[1] Preyer, W., *The Senses and the Will*, 1888, p. 236.

[2] Morgan, Lloyd, *op. cit.*, p. 36.

[3] Breed, Frederick A., "The Development of Certain Instincts and Habits in Chicks," Part I, *Behavior Monographs*, Vol. I, 1911–1912, pp. 1–41.

[4] See Breed, F. S., *op. cit.*, pp. 23–24.

[5] *Op. cit.*, pp. 36–37.

[6] *Op. cit.*, pp. 17–18.

[7] "The Instinctive Reactions of Young Chicks," *Psychological Review*, Vol. VI, 1899, p. 284; reprinted in *Animal Intelligence*, p. 159.

[8] *Loc. cit.*

[9] *Psych. Rev.*, Vol. VI, 1899, p. 285.

young chick's congenital capacity for spatial perceptions and reactions based thereon. We should observe at the outset that this capacity involves a considerable number of hereditary adaptations to the environment, since the chick's reactions have reference to objects of various sizes situated at various distances from it. Moreover, its estimates of distance unquestionably depend on some sort of coördination of visual sensations of various qualities and the *movements* requisite to the traverse of distances separating the percipient organism, or its parts, from the sources of the stimuli for those various sensations. For visual sensations alone cannot constitute the basis for estimations of distance, or for discriminations between distances associated with visual sensations of various qualities.

The germinal hypothesis (either form thereof) must explain these active adaptations to the spatial world on the supposition that a variation determining each sort of perception and reaction for which the chick has a congenital capacity occurred at one time or another in the germ-plasm of the chick's ancestors.

Now, however the germinal determinants of the chick's hereditary capacity for spatial perceptions and reactions be conceived, there can be no question that these determinants represent in some sense specific objects and relationships of the spatial world. The chick's reactions indicate that a large number of qualitatively different perceptions are congenitally determined when the appropriate stimuli are presented. Such qualitatively different perceptions must be numerically equal to the different combinations of objects of various sizes (perhaps of different shapes and colors as well) with various distances, which are discriminated by the chick, as indicated by its qualitatively different reactions to such combinations. The chick's perception is based in part on an extraordinarily delicate discrimination between various visual sensations, as its different reactions to distances of ten and sixteen inches, respectively, from the ground (Thorndike's box experiments) clearly demonstrate. Moreover, the germinal determinants involved represent kinæsthetic sensations as well, since series of such sensations must be compounded, so to speak, with visual sensations before the latter can represent distances or provide the basis for distance discriminations. That the two—visual and kinæsthetic sensations (or vision and movement)—are bound together phylogenetically, in the chick's case, is demonstrated by the fact that the chick's movements are inseparably connected with its visual perceptions, when the latter are possible to it.

It would scarcely be going too far to say, in view of these considerations, that the chick's germ-plasm represents in some sense the typical

experience or functional activity of its species in the spatial world, since its hereditary characters determine, apart from prior experience, activities the same in kind as those based on, and constituting, the reactions of its species to the objects and relationships of the spatial world. This equivalence of acquired responses to objects in space and hereditary responses to objects of the same kind is so nearly perfect that, if we can exempt these phenomena from anti-Lamarckian interpretations, we shall have empirical evidence of a very direct and cogent sort supporting the Lamarckian principle. Let us see if this can be done.

As aforesaid, the chick's germinal determinants represent numerous specific features of the environment, as demonstrated by its reactions to various complex combinations of visual stimuli (light waves). (To simplify the analysis, let us disregard the bearing of kinæsthetic sensations on the problem under consideration.) Now, there is nothing in the germinal hypothesis which can account for variations arising which represent, or have explicit reference to, complex specific features of the external world, features of which the germplasm itself could have had no experience. In order to account for the fact that the chick's germinal determinants actually do fit and represent specific features of the external world, the advocates of the germinal hypothesis, when pressed to do so, must postulate a preëstablished harmony between the two, or else abandon their hypothesis altogether, as applied to such cases.

The external agency providing such preëstablished harmony between the chick's germinal determinants and specific features of the spatial world would also be required to control the order in which the variations having a spatial reference appeared. For many different sorts of spatial perceptions, with the reactions thereto, are congenitally determined in the chick, and these taken together constitute what might be termed a highly adaptive *system* of spatial perceptions. "Selection" could hardly preserve chance variations determining single kinds of spacial perceptions, as, for example, perceptions of objects 10, 20 or 100 cm. distant. Such isolated variations might occur generations apart, and could not, in any case, occur in such a way as to constitute a system of spatial perceptions, without the good offices of some coördinating agency of the sort indicated.

Should we grant that germinal variations could fit specific features of the spatial world, the chance is negligible that the accidental variations occurring should fit specific complex combinations of visual stimuli (light waves), rather than other specific features among the all but *infinite* number of such features which constitute the environ-

ment. A germ-plasm having to produce *on the average* an almost infinite number of variations fitting respectively the same number of complex features constituting the environment, before the complement of variations occurred which actually adapted it to the environment, would be totally unfit for survival. But, according to the implications of the germinal hypothesis, all germ-plasms are in just this predicament.

We may conclude from the foregoing analysis that the advocates of the germinal hypothesis cannot, in a case of this kind, account for the occurrence of the required variations, or the requisite order or system of variations, without so construing their hypothesis as utterly to destroy it. They must postulate either (1) a preëstablished harmony between the given germinal variations and specific features of the environment to which they have reference, and an external agent to provide this harmony and to coördinate those variations in the requisite manner; or (2) a germ-plasm which turns out to be totally unfit for survival, because of its indiscriminate and hence unadaptive variability.

We have throughout considered germinal changes, as interpreted on the non-chemical form of the germinal hypothesis, in a qualitative sense, although Weismann himself favored the quantitative conception of such changes. We have done this in order to give the hypothesis the benefit of its most favorable construction, for the quantitative conception of germinal changes meets with special difficulties not involved in the alternative qualitative conception. Weismann at times conceded the possibility that many variations might be due to chemical reactions in the germ-plasm, and changes of this nature would of course be interpreted qualitatively. It may be well to consider the question whether the general hypothesis would be placed in a more favorable position, relative to active adaptations, by combining it with the quantitative conception of germinal changes.

It might be supposed, at first sight, that a single or a very few germinal determinants could have equipped the chick's ancestors for a rudimentary sort of spatial perceptions and reactions, and that these could have developed by growth alone (through the process of germinal selection postulated by Weismann) into the elaborate hereditary capacity for spatial perceptions and reactions of the present-day chick.

But an examination of this subsidiary hypothesis will demonstrate its inapplicability to hereditary characters of this sort. To begin with, a considerable number of qualitatively different reactions to spatial situations are discernible in the chick's behavior. How large the num-

ber of such reactions is, only an exhaustive study of its behavior could determine. These different reactions are not reducible to terms of one another. The chick does or does not peck at objects of various sizes and colors, situated at various distances away, and these reactions are not qualitatively the same. Pecking at a grain of rice on the experiment table could not be regarded as a reaction the same in kind as not pecking at a large black object ten or fifty feet away. Again, the reactions of the same chick, in the box experiments, to distances from the ground, of ten, sixteen and thirty-nine inches, respectively, are quite different in kind. All these qualitatively different reactions must have qualitatively different determinants in the germ-plasm, whatever our conception of the germ-plasm and its constituent determinants may be. The germinal hypothesis, and indeed all other scientific hypotheses dealing with heredity, must assume a germinal determinant of some sort for each qualitatively different perception and reaction exhibited by the young chick, which is not due to its own learning processes. No such determinant could produce, by way of its own diminution or enlargement, a determinant of a different kind.

Nor to employ concrete illustrations, can we see how the possession of a determinant for the jumping-down reaction at a height of ten inches could make more probable the genesis of a variation determining the opposite reaction at a height of thirty-nine inches, or of a determinant for the pecking reaction to small white objects, as grains of rice placed within pecking distance. All these different reactions are correlated with different specific features of the environment, or with different combinations thereof, and we do not see that the germinal determinant for one of these reactions could facilitate the genesis of another determinant for a different reaction: they are qualitatively distinct, and represent distinct features of the environment.

A possible qualification of this analysis may be noted. An animal possessing an end-organ of vision may be assumed to have a greater capacity for germinal variations modifying its visual functions than an animal not possessed of such an organ. We believe that different phyletic series do possess diverse capacities for the development of specialized and, in some sense, qualitatively different functions; and that the origin of such capacities cannot be interpreted in terms of functional activity or of physicochemical reactions alone, or in terms of both types of processes combined. But such capacities must, on our analysis, be regarded as potentialities only, and not as developed functions, for the latter cannot exist or be conceived apart from the specific

features of the environment to which they have reference. A given species or group therefore only has a greater *capacity* or *potentiality* for variations of a certain kind—variations relating to vision, for example—than has another species or group not possessed of the requisite capacity for such variations. But in the genesis of particular specialized functions, such as the qualitatively different perceptions and reactions of the young chick, the coöperation of specific features of the environment by way of the organism's functional activity is essential.

We may conclude, therefore, that the germinal hypothesis cuts itself off from the advantages which would accrue to it from the assumption of a preëxisting germinal organization favoring the occurrence of hereditary variations of a certain kind, through its refusal to accept the coöperation of specific features of the environment implied by such variations. But the acceptance of that coöperation in the sense here considered would transform it into the opposing hypothesis. Its difficulty rests at bottom on the exaggerated emphasis which it gives to primordial organizatory factors in vital phenomena, and its virtual exclusion of the environment and of functional activity from any share in the genesis of variations.

The general conclusion from the foregoing discussion is that the difficulties of the germinal hypothesis are not mitigated by a substitution therein of the quantitative for the qualitative conception of germinal changes, nor by taking into account the diverse capacities of different species for hereditary variations of particular kinds. The same analysis will apply, with the necessary qualifications, to the interpretation, on the germinal hypothesis in its quantitative form, of the ladybird's adaptation to the cottony cushion scales, as well as of adaptations to temperature and other features of the medium.

We shall not interpolate a consideration of the germinal hypothesis in its chemical form, as applied to the chick's spatial adaptations, since its interpretation of the case must be practically the same as that yielded by the non-chemical form of the hypothesis. As before, it meets with the additional difficulty of accounting for the organization implied by the adaptations in question, a difficulty of a sort which, as we shall see later, is of the most serious consequence to this type of hypothesis.

A brief consideration of other anti-Lamarckian hypotheses, as applied to this case, will suffice for our purposes, as their interpretations of the case would parallel interpretations, on the same hypotheses, of cases already analyzed, and our criticism of those interpretations would therefore be virtually the same.

That the external stimuli involved in the genesis of the chick's spatial adaptations could have established those adaptations through a direct action on the germ-plasm of the chick's ancestors is just as improbable as we saw the same hypothetical process to be in the case of the ladybird's adaptation to the cottony cushion scales. The environmental factors must have been much the same in the two cases, visual and perhaps (in the ladybird's case) olfactory stimuli being the primary factors of this category operative therein. Doubtless there were differences in the secondary stimuli involved, as in the gustatory, cutaneous, organic and kinæsthetic sensations probably operative in the two cases; but these differences would not greatly differentiate the interpretations of the two. Indeed, the parallel induction hypothesis could not consistently appeal to such secondary stimuli as being auxiliary factors in the genesis of hereditary adaptations to the primary stimuli, as the secondary stimuli partially constitute the *somatic* responses to the primary stimuli, and could not be regarded as external factors. It is, in fact, just the hereditary determination of those secondary stimuli, together with other features of the somatic response to the primary stimuli, that the parallel inductionists attempt to explain wholly in terms of direct action by the primary stimuli. In any case they have no way of accounting for the parallelism between the germinal or hereditary modifications induced by the external stimuli, and the somatic or acquired modifications induced by the same stimuli, except by invoking some outside agency capable of determining such parallelism.

The inapplicability of the hypothesis in question to this case becomes evident when the differentia between stimuli determining qualitatively distinct perceptions and reactions are taken into account. Consider, for example, the slight differences between the stimuli affecting the chick when placed on boxes ten and sixteen inches high, respectively, *and* the exclusion of the major portion of these differences from the chick's germ-plasm, by virtue of the small penetration coefficients of such stimuli. It should be clear from such considerations that any rational interpretation of the different hereditary reactions to these stimuli must rest on the assumption that the latter came to be discriminated by the chick's ancestors through highly efficient stimulus-receptors, and that the capacities for such discriminations and for the correlative responses gradually impressed themselves in some way on the germ-plasm of the chick's ancestors. The inference that the responses correlated with capacities for spatial perceptions became hereditary with the latter, follows as a corollary of the proposition that the first

step in the genetic process under consideration was a somatic and not a germinal discrimination between the primary stimuli involved, the uniform connections between the primary stimuli and the correlative somatic responses being taken for granted.

If the parallel induction hypothesis is incapable of accounting for the genesis of two qualitatively distinct reactions correlated with complexes of environmental stimuli but slightly differentiated from each other, it could scarcely account for the genesis of spatial perceptions and reactions correlative with complexes of stimuli more differentiated. Were it so employed, we should have two distinct principles applied to hereditary adaptations in the same category, and this would demand explanation. Moreover, an exhaustive analysis of the chick's spatial perceptions and reactions would probably yield the possibility of arranging these perceptions and reactions in a qualitative series, each member of which stood very close to the contiguous members of the series, as regards the complexes of environmental stimuli with which they were correlated. Our prior analysis would then apply to reactions occupying contiguous positions in the series, and, as a consequence of such application, to the series as a whole.

All this on the supposition that the parallel inductionists *might* interpret this case on their hypothesis, not that they *would* necessarily attempt to do so. The favored anti-Lamarckian interpretation of the case would probably be drawn from some form of the germinal hypothesis properly so-called.

Our criticism of the interpretation of this case on the non-representative transmission hypothesis would be identical with that directed against its interpretation of other cases, and need not be repeated. As in those cases, an external agent would have to be invoked in order to account for the equivalence of the series of functional modifications in the soma, and the series of functional modifications determined by germinal variations, which, according to the hypothesis, would be non-representative effects of functional activity.

The only alternative explanation remaining is that supplied by the Lamarckian hypothesis, in the modified form here entertained. As before, this hypothesis is supported both by the exclusion, as explanations, of the opposing hypotheses, and by the similarity between the chick's hereditary perceptions and responses, when appropriate stimuli are presented to it, and perceptions and responses having an experiential basis. Our previous remarks respecting various objections which may be urged against our analysis of active adaptations apply to the present case and need not be repeated here. These objections will be fully considered in later chapters.

Reciprocal Adaptations Between Different Species

The last type of adaptations which we shall employ as a test of Lamarckian and anti-Lamarckian hypotheses comes within the general category of parasitism, symbiosis and commensalism.[1] In Caullery's words, "parasites more than any other category of organisms, are a collective and striking illustration of adaptation. Nowhere else does structure appear so sharply outlined as modeled by the kind of life, nor does preadaptation appear less probable." [2] Moreover, this class of adaptations are widely distributed in the plant and animal kingdoms, very few groups being without species in a parasitic condition. The Hymenoptera alone has over 200,000 parasitic species, while a large proportion of Crustacea, Arachnida, Mastigophora, Sporozoa and Infusoria, and a majority of the Fungi spend all or part of their lives in a parasitic condition.

Parasitic adaptations are most diverse, as "parasites tend to become so specialized as to be peculiar to particular hosts; ectoparasites frequently differ from species to species, and the flea of one mammal, for instance, may rapidly die if it be transferred to another although similar host. The larval and adult stages of endoparasites become similarly specialized. . . . The general tendency is in the direction of absolute limitation of one parasite, and indeed one stage of one parasite to one kind of host. The series of events seems to be a gradual progression from temporary or occasional parasitism to obligatory parasitism and to a further restriction of the obligatory parasite to a particular kind of host."[3]

Moreover, the action of parasites "is frequently selective; particular substances, such as glycogen, are absorbed in quantities, or particular organs are specially attacked, with a consequent overthrow of the metabolic balance. Serious anaemia out of all proportion to the mass of parasites present is frequently produced, and the hosts become weak and fail to thrive. A. Giard has worked out the special case which he has designated as 'parasitic castration' and shown to be frequent amongst animal hosts." [4] More familiar instances of such speciali-

[1] The principal sources relied on for typical facts relative to such adaptations are: Caullery, Maurice, "Parasitism and Symbiosis in Their Relation to the Problem of Evolution," translated from *Revue Scientifique,* 1919, pp. 737–745, and published in the *Annual Report of the Smithsonian* Institution, 1920, pp. 399–409; Mitchell, Peter Chalmers, "Parasitism," *Encyclopedia Brittanica,* 11th edition, 1911, Vol. XX, pp. 793–797; Herms, W. B., *A Laboratory Guide to the Study of Parasitology.*

[2] *Annual Report of the Smithsonian Institution,* 1920, p. 406.

[3] Mitchell, P. C., *op. cit.,* p. 796.

[4] Mitchell, *loc. cit.*

zation are represented by infectious diseases in man that are caused by the attacks of parasitic organisms on particular organs.

Some parasites may be restricted to a single variety of a species and unadapted to other varieties of the same species. "Thus *Gonospora longissima,* a gregarine which Mr. Mesnil and I have studied,* is always present and abundant in one of the forms of *Dodecaceria concharum,* the form which we have designated as B; it is never found in form A; and yet these two forms represent a single species of annelid, or two species which are very nearly related." [1] Moreover, to quote Caullery further, "Giard asserted with great plausibility, it seems, . . . that two similar parasites found on related species are actually distinct, even when, morphologically, we are unable to discover definite differences of structure or form." [2]

With the development of the parasitic condition goes a progressive modification of hereditary structure. "Organs of prehension are notably developed; parasitic plants have twining stems, boring roots and special clinging organs; parasitic animals display hooks, suckers and boring apparatus. The normal organs of locomotion tend to disappear, whether these be wings or walking legs. Organs of sense, the chief purpose of which is to make animals react quickly to changes in the environment, become degenerate in proportion as the changes which the parasite may have to encounter are diminished. . . . The animal has no longer to seek its food, and the lithe segmentation of a body adapted for locomotion becomes replaced by a squat or insinuating form. Jaws give place to sucking and piercing tubes, the alimentary canal becomes simplified, or may disappear altogether, the parasite living in the juices of its host, and absorbing them through the skin. So, also, parasites obtaining protection from the tissues of their host lose their intrinsic protective mechanisms." [3] So profound are these structural modifications in many parasites, that their affinities have become unrecognizable.[4]

Contrasted with this extensive degeneracy of species in an advanced stage of parasitism, is a concomitant development of their egg-producing capacity. Leuckart estimated, for example, that the human tapeworm with an average lifetime of only two years produces approximately eighty million eggs. This development of the egg-laying capacity is obviously necessitated by the limited medium to which an

[1] Caullery, M., *op. cit.,* p. 403. The reference in the passage quoted is to Caullery and Mesnil, "Les formes épitoques et l'évolution des Cirratuliens," *Ann. Univ. Lyon,* fasc. 38, 1898.

[2] *Op. cit.,* p. 404.

[3] Mitchell. P. C., *op. cit.,* p. 796.

[4] Caullery, M., *op. cit.,* p. 405.

obligatory parasite is adapted. As Mitchell puts it, "a creature rigidly adapted to a special environment fails if it does not reach that environment, and hence species most successful in reproduction are able to afford the largest number of misses to secure a few hits and so to maintain existence." [1]

Species serving as hosts to parasites have met the attacks of the latter by developing a great variety of protective adaptations. "Such adaptations range from the presence of thickened cuticles, and hairs or spines, the discharge of waxy, sticky or slimy secretions, to the most elaborate reactions of the tissues of the host to the toxins liberated by the parasites." [2] Included among protective adaptations of this sort are the immunities to various infectious diseases possessed by one or another race of our own species. That such immunity is gradually acquired, through exposure to and defensive reaction against the given disease, is indicated by the fact that a race suffers more severely from an infectious disease when first exposed to it than at a later period.

We are not here discussing the genesis of such protective adaptations, but only citing instances which tend to show that parasitic adaptations often, and perhaps generally, induce compensatory adaptations on the part of the species serving as hosts. Indeed, we are logically forced to the conclusion that such compensatory adaptations must always be developed, if the parasitic attacks are of such a nature as to cause the death of the host, in the absence of such adaptations. Otherwise, the host species would be doomed to eventual extinction, as would also the parasite, if rigidly adapted to that particular host.

The relation between two species reciprocally adapted to each other may be one of mutual advantage rather than one of attack and defense. This condition, known as symbiosis, is also widely distributed in nature. Commensalism, by contrast, refers to reciprocal adaptations in which neither party to the relationship derives any marked benefit from it. Examples of symbiosis and commensalism are to be found in the human intestine. Many harmless organisms flourish there, while certain species of bacteria aid in intestinal digestion. An oft cited case of symbiosis is that obtaining between hermit crabs and sea-anemones, the former providing the latter with food, and receiving, in return, protection afforded by the stinging threads and nettle cells of the latter. A specific example of commensalism is the union of the glass sponge (*Euplectella*) with commensal crustacea. Parasitism, symbiosis and commensalism are not sharply distinguished from one another, but are to be regarded as subdivisions of the general class of

[1] *Op. cit.*, p. 796.

[2] Mitchell, P. C., *op. cit.*, p. 797.

adaptations involving the communal existence of distinct species, together with various antagonistic or coöperative relationships between those species.

We may summarize briefly the facts from this class of adaptations that will serve as a test of our hypotheses respecting the origin of variations.

(1) There is, in the case of obligatory parasitism, a rigid hereditary adaptation of one species to another species or to a small number of species, and probably in some cases to a single variety of another species. The tendency is for such adaptations to be further restricted to single organs of the host, as in parasitic castration, or in specific infectious diseases. Some parasites are, however, rigidly adapted at one stage of development to one host species, and at another stage of development to a different host species. This is true, for instance, of the pork tapeworm found in man (*Tænia solium*). A species may also possess a hereditary adaptation of the symbiotic or commensal type, to another species or to several species. We shall largely restrict our analysis of these cases to hereditary parasitic adaptations having reference to a single species or variety, together with the reciprocal adaptations on the part of species or varieties serving as hosts.

(2) The obligatory parasitic adaptation is usually accompanied by a more or less *extensive* modification of the parasite's hereditary constitution. Symbiotic and commensal relationships connote similar modifications, the extent thereof depending on the nature of the given symbiotic or commensal relationships.

(3) Parasitic, symbiotic or commensal adaptations are often if not generally correlated with adaptations in the species serving as hosts or as the commensal or symbiotic partners. These compensatory or reciprocal adaptations represent more or less extensive modifications of hereditary structure and functions on the part of the host or partner, the actual extent thereof depending on the specific type of relationship involved.

The hereditary characters falling within this general category include reciprocal adaptations between what may be termed unique and irreproducible features of the organic world, each of them being the product of a historical sequence of causes which could not be repeated. Each of these reciprocal adaptations also represents, on one or both sides, an indefinite number of germinal changes coördinated into a determinate order or system.

When the facts of such cases are thus formulated, our analysis of other types of active adaptations, in relation to Lamarckian and anti-Lamarckian hypotheses, obviously becomes applicable, and we may

excuse ourselves from traversing that analysis in detail, with reference to the cases in question. It will suffice if we apply the results thereof to special features of these cases. We shall first consider, in this connection, the non-chemical form of the germinal hypothesis. The application of those results to the interpretation of these cases on that form of the hypothesis yields the following conclusions:

(1) A preëstablished harmony between germinal variations and specific features of the environment, in order that the correlations between the two may be accounted for, must be postulated by the germinal hypothesis.

(2) An external coördinating agency must be postulated not only to account for such preëstablished harmony but also for the order or system of germinal variations represented by most, if not all, of the active adaptations now under consideration.

(3) The representation of a unique and irreproducible feature of the environment, as implied in every parasitic, symbiotic, or commensal adaptation, is inadmissible, even if the foregoing assumptions be granted, since representation of such features of the environment, without prior acquaintance with or reactions to those features, is a self-contradictory conception.

These are particularly drastic conclusions, in reference to the cases here under consideration, since an indefinitely large number of germinal modifications are involved in the evolution of an obligatory parasite rigidly adapted to a single species or variety. These conclusions are still more drastic, when considered in relation to an obligatory parasite, such as *Tænia solium,* whose hereditary constitution has been profoundly modified during its parasitic evolution, and which is now rigidly adapted, at different stages of its existence, to two species serving as hosts.

These conclusions apply to the interpretation, on the germinal hypothesis, of the hereditary adaptations in *one* of the parties to the parasitic, symbiotic or commensal relationship. When the reciprocal adaptations of *both* parties are interpreted on the same hypothesis, the enormity of the implied empirical consequences is greatly accentuated. We should have, for example, the spectacle of the host's germ-plasm undergoing modifications having an explicit reference to the parasite's adaptations to that same host, and without functional activity playing any representative rôle in the process! In other words, in evolving a protective adaptation, the host's germinal variations would *represent* the parasite's *representations* of the host, and without prior reaction of one to the other being involved in any part of the process. And all the other logical consequences which follow from the germinal

interpretation of the genetic process in one party to the parasitic, symbiotic or commensal relationship become similarly magnified when the interpretation is extended to the genetic processes in both parties thereto.

We might, with the aid of symbols, construct a mathematical formula representing the chances of reciprocal adaptations between an obligatory parasite and its host occurring according to the process postulated by the germinal hypothesis.

Let us assume that germinal variations, as conceived by the hypothesis, *could* represent other species of organisms. Let us assume, further, that the chances of their representing the various species of organisms are equal, an assumption which logically follows from that hypothesis as formulated by its advocates. We may disregard for the present the equal chances of variations occurring which would represent specific features of the environment other than species of organisms. Let the total number of species (past, present and future) be represented by the letter m. Let us suppose that in a given species which became parasitic, n variations in a definite order were required before the parasitic adaptation to the host species was complete. (On the germinal hypothesis, such variations are not more likely to occur in any one order than in any other.) Let n' be the number of germinal variations that had to occur in the host species, in a definite order, before its protective adaptation to the parasite was complete.

On these assumptions, the formula representing the chance of the given reciprocal adaptations occurring would be

$$\frac{1}{m^2\,(\underline{|n})\,(\underline{|n'})}.$$ [1]

No numerical value can of course be computed for this fraction, but considering the probable values of m, n and n', it can safely be put down as infinitesimally small. There is no doubt that the denominator of the fraction thus symbolically represented would in many if not most of the reciprocal adaptations under consideration *exceed the total number of organisms that have existed in the world.* This anyone interested may test for himself by computing a numerical value for the denominator, based on numerical values for m, n and n', falling well within the probable facts of such cases.

But in order to construct this formula concessions were made to the germinal hypothesis, for which there is no justification. There is no possibility, if our previous analysis be correct, that the hereditary characters of an organism should represent specific features of the

[1] $\underline{|n}$ or factorial n is the symbol designating the function, $2 \times 3 \times 4 \ldots \times n$.

environment, and particularly those unique and irreproducible features designated as species, varieties or whatever taxonomic term we may apply to them, without a prior interaction or acquaintance with those environmental features. Nor are there any grounds for the concession implied in the formula, that the individual germinal variations included in *n* or *n′* could be retained until adaptive combinations thereof were effected. When these concessions are withdrawn the germinal hypothesis is really reduced to a verbal formula without any assignable content whatever.

The foregoing analysis requires little if any qualification to make it fit the chemical form of the germinal hypothesis. It is probable that chemical conditions are important factors in the evolution of parasitic species, as the profound modifications correlated with the appropriation of nutritive substances from the host species would imply. Considering the fact, however, that specific differences between parasites are of an incomparably wider range than any demonstrable differences in the chemical compositions of their germ-plasms, it could hardly be maintained that the adaptations peculiar to the parasitic condition are to be interpreted in chemical terms alone.

Evidence tending to support the same conclusion is supplied by the absence of any correlation between parasitic, symbiotic or commensal relationships and the taxonomic positions of the species functioning in these relationships as partners, or as parasites and hosts. While it is true that the hosts of many parasitic Hymenoptera are members of an order in the same class, the Lepidoptera, many parasites and their hosts are in widely separated phyla and even in separate kingdoms. The pathogenic organisms preying on the human species and the bacteria contributing to intestinal digestion are cases in point. If chemical changes in the germ-plasm were directly responsible for any of the specific differences between parasitic, symbiotic and commensal adaptations, some sort of parallelism between the incidence of those adaptations and the taxonomic relationships of the parties thereto would seem to follow as a logical consequence.

That chemical factors are involved in all hereditary modifications, we have repeatedly insisted. But specific active adaptations as such are not interpretable in chemical terms alone. Neither the organization represented by such adaptations nor the organizations (species or varieties) to which, in these cases, the adaptations refer can be translated into chemical terms. The truth of that proposition will be demonstrated, we believe, in a later part of the discussion. Moreover, a species adapted to specific features of the environment *must be defined, in part, in terms of those specific features,* and in terms of unique

and irreproducible features of the environment, where the adaptations are to other species of organisms. Chemistry has no principles whereby to account for relationships of this order, and indeed no language whereby they may be described. We must conclude, then, that the special chemical conditions involved in the evolution of parasitism may facilitate in some directions, and limit in others, the genesis of adaptations peculiar to this condition, but that they cannot account for the adaptations themselves, when regarded as representations of specific features of the environment.

But little need be said regarding the possible interpretation of these cases on the hypothesis of parallel induction. The analysis of this hypothesis in relation to the ladybird's adaptation to the cottony cushion scales will apply in a general way to this group of cases, since the phenomena are in some respects similar. The parallel between that adaptation and the initial stages of parasitism is indeed rather close, since there is a similar food adaptation in the two cases. One series of differences may be noted. The stimuli operative in all but the initial stages of parasitism would be various fluids, juices, tissues or other nutritive substances afforded by the host, the absence of light from the medium of the entoparasites, and tissues of the host which the given parasite might need to penetrate or attach itself to in one way or another. Regarding these stimuli, we may say that the mere presence of the host's tissues could not directly induce germinal variations giving rise to prehensile organs or sucking or piercing tubes adapted to those tissues; that, although the juices or fluids of the host might be deemed capable of acting directly on the parasite's germ-plasm, they could scarcely determine a gradual modification of the alimentary canal, for example, or its disappearance altogether, as any direct action of such fluids could scarcely be as orderly and as adaptive as that; and that the *absence* of light would hardly be capable of inducing any such changes.

In addition, there is the difficulty which confronts all genetic interpretations on the hypothesis of parallel induction, that, namely, of accounting for the equivalence of the results produced through the soma and germ-plasm, respectively, by the environmental stimuli involved in hereditary modifications. The insupportable logical consequences of the hypothesis, as revealed in our analysis of other cases, together with the obvious impossibility that certain types of environmental stimuli involved in hereditary modifications could produce those modifications through direct action on the germ-plasm, invalidate the hypothesis as an explanation of active adaptations representing specific features of the environment.

Our criticism of the non-representative transmission hypothesis in relation to other cases of adaptation applies to the present group of cases also, and need not be repeated in connection therewith. To consider one concrete case only, it is impossible to see how any non-representative effect of functional activity on the hereditary constitution of an organism could determine immunity to specific infectious diseases. In the absence of evidence to the contrary, we must assume that the reactions of immune or partially immune races to parasitic organisms causing those diseases are the same in kind as, though more effective than, the original defensive reactions to the same diseases. So far as a layman can judge, the experimental data on immunity support this conclusion. The objections to the hypothesis under consideration, as set forth in connection with other types of active adaptations apply, without any special qualifications, to these and other specific instances of reciprocal adaptations between parasite and host. The equivalence of the functional activities in the soma with those determined thereby in the offspring can be accounted for, on the non-representative transmission hypothesis, only by postulating some extra-organic principle or agency to provide the requisite coordinations.[1]

The Lamarckian hypothesis (in the form entertained here) is therefore the only hypothesis available as an interpretation for this group of adaptations. As in previous cases, it is supported negatively by the exclusion, as explanations, of the opposing hypotheses, and positively by the facts of the case when freed from the logically indefensible interpretations put upon them by the opposing hypotheses.

We may take from Herms a concise statement of the more significant facts of the case. "Modern parasites are restricted more or less completely to a particular host animal, which would necessitate the deduction that the parasite must have developed its habit after the existence of the host, and in consequence that parasitism must be a recently acquired habit. This thought is further expressed by the study of the life history of the parasite. Invariably the earlier stages point to a free living existence. Perhaps the ancestors of a given group of modern parasites were attracted to the waste food, offal and exudations of certain animals; the search for food may have become simplified; they began living as messmates or commensalists, or as scavengers; the association between the species may have become closer and the eventual line of parasitism completed. This is also borne out by a study of the nearest allies of a given parasite and members of a given family of

[1] For a discussion of immunity from a somewhat different angle, see Chapter V.

parasites, in which the gradation from free living animal to parasite may often be traced." [1]

The Lamarckian hypothesis affords the simplest interpretation of these facts, and the only one that does not meet insuperable logical difficulties. Subject to qualifications already stated in relation to other cases, the foregoing analysis may reasonably be deemed to reëstablish the Lamarckian hypothesis as applied to this group of active adaptations.

Combinations of Anti-Lamarckian Hypotheses

Let us see, before taking up the consideration of intra-organic adaptations, whether the several anti-Lamarckian hypotheses could in combination account for active adaptations of the organism as a whole to specific features of its environment.

We may observe, in respect to such a combination, that a tenable hypothesis could hardly be produced by compounding together a number of untenable hypotheses. If the germinal, parallel induction and non-representative transmission hypotheses all meet with insuperable difficulties in attempting to account for the genesis of active adaptations, they would meet with just the same difficulties when applied collectively to the same problem. These several hypotheses would not lose their identity through a combination thereof into an offensive or defensive alliance, if we may so speak, but would each carry the same empirical implications as when standing alone. Moreover, these several hypotheses are more or less exclusive of one another, if regarded as supplying genetic interpretations of the same hereditary characters.

As conceived by their exponents, the processes respectively postulated by the germinal and the parallel induction hypotheses are distinct from each other. While both must assume that germinal changes originating on either process involve functional activity within the germ-plasm, this activity itself, together with the changes to which it leads, is assumed to be induced either by environmental stimuli or by factors intrinsic to the existing germinal organization, nutritive substances and other conditions necessary to functional activity within the germ-plasm being taken for granted. Conceding the possibility that initiatory agencies of the two types could coöperate together in the genesis of the same germinal variations, and there is perhaps nothing in such a concession inconsistent with either of the two hypotheses, our criticism of

[1] Herms, W. B., *A Laboratory Guide to the Study of Parasitology*, pp. xiii–xiv. Copyrighted 1913, and quotation authorized, by The Macmillan Company. For a similar account see Mitchell, P. C., *op. cit.*, p. 794.

the combined hypotheses, when employed as interpretations of active adaptations, would be identical with our criticisms of them, in the same connection, when taken separately.

The non-representative transmission hypothesis is of course opposed, in principle, to both the hypotheses just adverted to, although it might be combined with either or both of them, if the constituent hypotheses in such a combination were taken in a modified form. The same observations apply, however, to possible combinations of this sort as to the combination just considered.

Perhaps a more serious criticism of such combinations would be, that they could not represent real syntheses of factors actually operative in phylogenesis, but only artificially isolated processes conceived to be operative therein, and mechanically juxtaposed together. The result would be an assemblage of hypotheses, each based on an erroneous conception of the organism, and each subject to a destructive criticism drawn from the facts which it ignores or minimizes. A truly synthetic theory, on the other hand, would not assume isolated systems in the organism not actually found there, or propose onesided hypotheses ignoring or discounting wide ranges of vital phenomena.

In any case we can conceive of no combination among these hypotheses which could account for the occurrence of hereditary variations in sufficient numbers to adapt species to their environments; the representation, by adaptive variations, of specific features of the environment; and the parallelism between adaptations to the environment determined by heredity and adaptations to the same features of the environment acquired by the ancestral organisms. Correlations of these types cannot be accounted for on any one or any combination of the anti-Lamarckian hypotheses, without the postulation of an external agency for this specific purpose. Such a postulation, however, would represent a repudiation of the evolutionary theory itself, and indeed of modern science, resting, as these do, on the assumption that natural phenomena are all to be accounted for in terms of factors within the phenomena themselves, free from interference by agencies external to such phenomena.

CHAPTER IV

LAMARCKIAN AND ANTI-LAMARCKIAN INTERPRETATIONS OF INTRA-ORGANIC ADAPTATIONS

We have reserved for a separate chapter our analysis of intra-organic adaptations, considered as tests of Lamarckian and anti-Lamarckian hypotheses respecting the origin of variations.

This part of the analysis must of necessity be couched in rather general terms, owing to the fact, singular enough, that the genetic interpretation of intra-organic adaptations has hitherto received comparatively little attention at the hands of the biologists. While it is true that the homologies between the constituent structures of various taxonomic groups have been worked out in some detail, the genesis of those structures has not been subjected to a systematic analysis approaching the same degree of elaboration. An enormous amount of work has been done on the "developmental mechanics" (*Entwicklungsmechanik*) of the organism, but the phylogenetic implications of the results attained thereby have received comparatively little consideration for their own sake.

Roux seems to have been the first to make a really serious beginning, largely theoretical in nature, on the general problem,[1] though early anticipations of the selectionist hypothesis, as applied to constituent structures of the organism, are to be found in the speculations of Empedocles and, much later, of Diderôt in his *Pensées sur l'Interprétation de la Nature*.[2] Roux's conception has met with some special applications at the hands of other biologists, notably in Weismann's hypothesis of intra-germinal selection, and, more recently, in Sherrington's work on the integrative action of the nervous system; but the more important elaborations of this conception have been in connection with developmental mechanics rather than the processes of phylogenesis. Recent work in genetics has also resulted in the accumulation of data available for theoretical researches on phylogenesis, although such work has hitherto been mainly of a descriptive character, while little or none of it has any direct bearing on the genetic interpretation of intra-organic adaptations.

[1] *Der Kamp der Teile im Organismus*, 1881.
[2] Osborn, H. F., *From the Greeks to Darwin*, pp. 52, 115-117.

Roux held that the most delicate adaptations of cells and tissues originate in a struggle with other cells and tissues, thus applying to parts of the organism the same principle (albeit in a qualified form), which had been successful, as it was thought, in accounting for adaptations of the organism as a whole. While Roux's theory has been cordially accepted by many biologists, it has been subjected to various drastic criticisms by other biologists; [1] yet the *problem* which engaged Roux's attention needs only be stated, for its farreaching significance to be made clear. He has, in short, raised the problem of histonal adaptations, and, as a simple corollary, the problem of intra-cellular adaptations as well. These problems are not exactly parallel with the problem of organismic adaptations, as Weismann has shown,[2] since, for example, the alleged struggle between parts, and the hereditary modifications said to be consequent thereon, are conditioned by the selective processes in which the organism as a whole is involved; but the problems are sufficiently related to permit at least a partial interpretation of intra-organic adaptations on the principles of organismic adaptation.

Everyone admits the interdependence of tissues and functions within the organism. It will be admitted also that each of them has an evolutionary history, that they are all descended from structures and functions not altogether like themselves. Nor will any one deny that every tissue bears certain necessary relations to an environment—to adjacent tissues and to the body as a whole; and that these relationships are, at least in part, hereditary. It will be admitted, further, that the cell has an evolutionary history all its own; that the component parts of the cell are coördinated together in definite ways; that the cell has a hereditary structure, and that each of its parts has an environment to which it is, by heredity, adapted.

Histonal and intra-cellular adaptations do not differ in kind, but only in degree, from adaptations of the organism as a whole to its environment. The former have merely become so complex as to constitute, when taken together, a higher degree of organization. But organismic adaptations constitute organization, too; only, in this case, organization is of a looser, more diffuse sort. There is less of rigid necessity, and a greater freedom of action, in responses to external stimuli by the organism as a whole, but the organism is none the less solidary with its environment, and constitutes, with it, a system or organization just as real as that which binds together the component tissues of the organ-

[1] See, for example, Plate, L., *Selektionsprinzip und Probleme der Artbildung,* 4th edition, 1913, pp. 337-351.

[2] *The Evolution Theory,* Vol. I, pp. 248 ff.

ism. We have here to do with differences of degree, rather than with differences of kind.

Any of the more exact definitions of instinct, such as that by McDougall,[1] for example, would apply, with necessary qualifications, to the interrelations of parts or structures in all these systems, whether it be the cell, the tissue or the organism and its external environment. There are in every case—cell-part, tissue, organism, etc.—*inherited* dispositions to react in *particular* ways to the situations, objects or stimuli constituting the given environment. Says Bergson: "When we see in a living body thousands of cells working together to a common end, dividing the task between them, living each for itself at the same time as for the others, preserving itself, feeding itself, reproducing itself, responding to the menace of danger by appropriate defensive reactions, how can we help thinking of so many instincts? . . . All goes on as if the cell knew, of the other cells, what concerns itself . . ." [2] These characterizations apply, with qualifications, not only to the cell but also to the cell-part, the tissue, the organ, the organ-system and the organism as a whole. This being so, our criticisms of the anti-Lamarckian interpretations of organismic adaptations will apply, with modifications, to the corresponding interpretations of intra-organic adaptations.

Intra-organic adaptations are correlated and at least partially identical with various mechanical, chemical and nervous reactions within the body, and many of them with two or all three types of reactions combined.[3] Intra-organic adaptations to be employed as further tests of

[1] *An Introduction to Social Psychology,* 13th edition, 1918, p. 30.

[2] *Creative Evolution,* pp. 166–167; Henry Holt and Company, publishers.

[3] A passage of Sherrington's presents significant instances of such adaptations, or types of integrative action, as he designates them. "The integration of the animal organism is obviously not the result solely of any single agency at work within it, but of several. Thus, there is the *mechanical* combination of the unit cells of the individual into a single mass. This is effected by fibrous stromata, capsules of organs, connective tissue in general, *e. g.* of the liver, and indeed the fibrous layer of the skin encapsulating the whole body. In muscles this mechanical integration of the organ may arrive at providing a single cord tendon by which the tensile stress of a myriad contractile cells can be additively concentrated upon a single place of application.

"Integration also results from *chemical* agency. Thus, reproductive organs, remote from one another, are given solidarity as a system by communication that is of chemical quality; lactation supervenes *post partum* in all the mammary glands of a bitch subsequent to thoracic transection of the spinal cord severing all nervous communication between the pectoral and the inguinal mammae (Goltz). In digestive organs we find chemical agency coördinating the action of separate glands, and thus contributing to the solidarity of function of the digestive glands as a whole. The products of salivary digestion on reaching the pyloric region of the stomach, and the gastric secretion on reaching the mucosa of the duodenum, make there substances which absorbed duly excite heightened secretion of gastric and of pan-

Lamarckian and anti-Lamarckian hypotheses will be cases in which processes of these several types are emphasized.[1]

Connective-Tissue Adaptations

Let us take the adaptations of connective tissue to parts of the body which it supports or holds together, as a type of intra-organic adaptations in which *mechanical* processes are of primary significance. Connective tissue is the "most widely distributed of the supporting tissues. It envelopes and pervades all the soft parts of the Body. The various constituents of individual organs are held together by it, and the organs themselves, are supported in their places by the same tissue. Beneath the skin and attaching it rather loosely to the underlying structures is a layer of connective tissue known as the fascia. So completely is the entire Body pervaded by connective tissue that if a solvent could be found which would dissolve away all the tissues of the Body except this one there would still remain in perfect outline not only the whole Body but also each organ down to minutest detail." [2]

A most perfect adaptation of connective to other tissues is here represented, and an adaptation which is undoubtedly hereditary. The

creatic juice respectively suited to continue the digestion of the substances initiating the reaction (Bayliss and Starling, Edkins). Again, there is the integrating action effected by the circulation of the blood. The gaseous exchanges at one limited surface of the body are made serviceable for the life of every living unit in the body. By the blood the excess of heat produced in one set of organs is brought to redress the loss of heat in others; and so on. . . .

"In the multicellular animal, especially for those higher reactions which constitute its behavior as a social unit in the natural economy, it is nervous reaction which *par excellence* integrates it, welds it together from its components, and constitutes it from a mere collection of organs an animal individual . . . its agent is not mere intercellular material, as in connective tissue, nor the transference of material in mass, as by the circulation; it works through living lines of stationary cells along which it despatches waves of physicochemical disturbance, and these act as releasing forces in distant organs where they finally impinge. . . . When the animal body reaches some degree of multicellular complexity, special cells assume the express office of connecting together other cells. Such cells, since their function is to stretch from one cell to another, are usually elongated; they form protoplasmic threads and they interconnect by conducting nervous impulses." Sherrington, C. S., *The Integrative Action of the Nervous System*, pp. 2–5; Yale University Press, publishers.

Sherrington does not in this passage raise the question whether integrative action by means of mechanical, chemical or nervous processes can be wholly accounted for in terms of such processes; or the further question whether integrative action has its origin exclusively in and through processes of these types. It is questions of this order with which we are primarily concerned.

[1] Typical adaptations about which there is little or no controversy may be employed for our purposes, and the analysis thereby made fairly independent of future experimental work in the general field.

[2] Martin, H. N. and E. G., *The Human Body*, 10th edition, p. 43; Henry Holt and Company, publishers.

myriad organisms (cells) which constitute the one class of tissues are all congenitally adapted to the myriad organisms which constitute the correlative class of tissues. And these are not adaptations *en masse,* but are specific for each part, perhaps for each cell. We must assume in the case before us that the correlative organs, tissues or tissue-parts which are capable of variation all have their determinants in the germ-plasm; and that all the corresponding variable units of the connective tissue have their germinal determinants, also. We are not supposing that all variations in the correlative tissues would necessitate concomitant variations in the corresponding connective tissues. But variations in the size, shape or weight of those tissues would necessarily involve variations in the correlated connective tissues.[1] Our discussion will have reference to adaptations implied by variations of these types.

Now, whatever we suppose the units of variation to be in the two classes of tissue, whether it be the single cell, cell-part or some rather large group of cells, the number of such units must be very large. We are to consider the case, thus presented, as a test of Lamarckian and anti-Lamarckian hypotheses respecting the origin of variations. Since we shall only be applying, in a modified form, the analysis developed in relation to organismic adaptations, we shall take the general results of that analysis for granted, and bring those results into connection with the case here under consideration. We shall examine first the interpretation of the case on the non-chemical form of the germinal hypothesis.

To simplify the problem, we will suppose—what must generally be the case—that the adaptation is *by* the connective tissue *to* the correlative tissues which it mechanically binds together. Stated in the briefest way, the interpretation of hereditary connective-tissue modifications on the hypothesis now under consideration carries with it the following consequences: (1) Modifications of connective tissues are adapted to specific features of the environment—in this case changes of the correlative tissues necessitating adaptive modifications of connective tissues—without any prior reaction thereto. To account for such adaptive variations and their occurrence in the requisite measure and order, some sort of preëstablished harmony between the germinal variations determining modifications of connective tissues, and changes in the correlative tissues must be postulated. (2) Unless this postulate be accepted, it could be shown, by a repetition of our previous analysis, that the chances of the requisite germinal variations occurring as needed are infinitesimally small; and that a germ-plasm with the variability requisite to the occurrence of such adaptive variations would be utterly

[1] Cf. Roux, W., *Der Kampf der Teile,* p. 39.

unfit for survival. (3) Since the tissues of a given species and the modifications thereof are unique and irreproducible features of the given environment, being the product of a historical sequence of causes which could not be repeated, it is not possible that the connective tissues could vary in such a way as to *represent* modifications of the correlative tissues, apart from prior reaction to those tissues as modified.

If we attempted to help the advocates of the germinal hypothesis out of their difficulties by assuming, for them, that the germinal determinants of somatic modifications are not independent of one another, but are organized into a system, we should do no more than push their problem and its difficulties one stage farther back. For the advocates of the hypothesis could not, consistently with their assumptions, claim that interadaptations or correlations between germinal determinants had been established through a functional coöperation of these determinants, but would necessarily hold that these had been determined by independent variations in some hypothetical germ-plasm of the germ-plasm, whereupon the same consequences would follow.

The chemical form of the germinal hypothesis apparently possesses no advantages over the non-chemical form of the hypothesis, as applied to this case, since the chemical changes assumed to be responsible for the adaptive variations in connective tissues must occur within the genes, factors or determinants of those tissues, which would involve virtually the same consequences as have just been detailed. This form of the hypothesis also meets with special difficulties indicated in other connections.

The hypothesis of parallel induction would probably not be proposed as an explanation of this case, but its possibilities in this connection may be considered. The only conceivable stimulus which might directly modify the germ-plasm in this case would be an increase or diminution of the tensile stress to which connective-tissue fibres are subjected, due to changes in the size, weight or shape of the correlative tissues. The stimulus in question, however, comes within the category of functional, not of environmental, stimuli. The only environmental stimuli involved in the case are changes in shape, size or weight of the correlative tissues, and these in themselves could not act on the germinal determinants of the corresponding connective tissues. The environmental stimulus must be transformed into a functional stimulus before it can have any effect on those determinants. And, considering the fact that these determinants are not themselves of a fibrous character (even though some of them be located in the connective tissues), the stimulus in question could hardly affect them *directly*.

Any explanation appealing to non-representative effects of functional activity is equally dubious. Indeed, we can form no clear idea of how such a process could be utilized in the genesis of adaptations of this sort. The functions of connective-tissue cells are of a very specialized sort, these being limited, apart from the maintenance of the cells themselves, to the production of connective-tissue fibres, together with the mechanical work of these fibres. In some cases, it is true, such cells give up this function and devote themselves to storing up fat; but these cells are not properly regarded as connective-tissue cells, once this transfer of function has taken place. Now, we may ask, what other functional activity of connective-tissue cells could determine the production of a greater or smaller quantity of connective-tissue fibres, when this is the only specialized function which may, strictly speaking, be attributed to them? The non-representative transmission hypothesis is evidently inapplicable to this case.

The only alternative remaining is the Lamarckian interpretation. Since, in the nature of the case, no empirical data are available whereby to test the various hypotheses which might be applied to this case, a decision as to the correct hypothesis must rest on evidence of an inferential nature. This evidence all goes to support the Lamarckian interpretation of the case. Besides the considerations just set forth, we may cite as further evidence of the same general character, the striking parallelism between the observed enlargement or diminution of a muscle through use or disuse, and the enlargement or elongation (or converse modification) of connective tissues due to change in the mass or dimensions of the correlative tissues. It is legitimate to infer that, in the latter case as in the former, the modification is induced by an increase or diminution in the tensile stress to which the tissues in question are subjected. The similarity of such somatic modifications to hereditary modifications of the same tissues supports the Lamarckian interpretation in a positive sense, when exempted from the interpretations yielded by the opposing hypotheses.

The reëstablishment of the Lamarckian hypothesis, as applied to this case, is conditional, for its ultimate validity, on the outcome of our examination, later on, of the specific and general objections which may be urged against the type of analysis here set forth.

Adaptations of the Digestive System

The digestive system in man embraces a complex series of intra-organic adaptations, relative to which *chemical* processes are very conspicuous and important, compared with mechanical or nervous proc-

esses; and we may employ these adaptations as a further test of Lamarckian and anti-Lamarckian hypotheses as to the origin of hereditary variations. It should be noted that this group of adaptations is not constituted exclusively of the chemical correlations between specific processes of digestion, but have reference also to the various tissues of the digestive system, and to functional activities thereof which do not enter into processes of digestion as such.[1]

This system of functional activity furnishes employment to a large number of organs, whose several activities are normally coördinated in a highly adaptive manner. We have the mouth-chamber, the tongue and the teeth; the pharynx and the esophagus; the stomach with its specialized structures—the fundus, the pylorus, the gastric glands, etc.; the blood-vessels serving the alimentary canal and its appendages, and so on. The histology of these parts distinguishes a much larger num-

[1] For convenience of reference, a brief standard account of digestive processes in man is herewith presented. This, in form, is the history of a meal.

"*In the mouth* the food is reduced to a semi-liquid alkaline mass, containing no large particles, by the combined action of chewing and mixing with the saliva. The salivary glands are reflexly excited to secrete their juice by the presence of the food in the mouth. The enzym of saliva, ptyalin, begins its digestive action on the starch, converting it to maltose. By the act of deglutition the food, when sufficiently mixed with saliva, is passed on to the stomach. If the chewing and swallowing of the food is attended with agreeable emotions, there is aroused a reflex secretion of gastric juice; the so-called 'psychical' secretion.

"The food *enters the stomach* in very much the same condition chemically as when taken into the mouth; a small amount of maltose added to it through the action of salivary ptyalin, and a correspondingly diminished amount of starch, being the only differences. That part of the food which is crowded down into the pyloric region begins at once to be churned by the peristaltic waves which sweep over that region; by the churning it is mixed with gastric juice. The food which remains in the fundic end of the stomach does not come into contact with the gastric juice; its reaction, therefore, continues alkaline, and the splitting of starch by pytalin goes on uninterruptedly. In the portion of food (chyme) which becomes impregnated with gastric juice there is an acid reaction and the changes which the gastric enzyms, pepsin, and rennin are capable of producing take place. Rennin clots any milk that may be present; pepsin attacks albuminoids and proteins, converting them into proteoses and peptones. Any fats present are liquefied, not by enzyms but by the stomach warmth. Some of the substances produced during this peptic digestion react with other substances in the mucosa of the pyloric region, forming a hormone, gastric secretin. This hormone is taken up by the blood, passes in the blood-stream to the gastric glands, and stimulates them to further outpouring of juice; thus enough for the whole meal is secured. Finally as the hydrochloric acid of the gastric juice accumulates in excess the pyloric sphincter is stimulated to relax; the mass of chyme next to it is pushed through; and more material from the fundic end comes down to fill its place. Too much chyme is prevented from passing the sphincter at once by the powerful stimulus to contraction which is exerted on the sphincter by the acid chyme in contact with the upper intestine. The acid of this same chyme reacts with the prosecretin of the intestinal mucosa to form secretin, a hormone which is carried by the blood to the pancreas and excites it to activity.

"The chyme which *enters the intestine* contains some, at least, of all the food

ber of specialized tissues, each with a particular function to perform. So *unified* is the whole system of structures and functions that it is awkward to speak of adaptations between particular structures; rather, each is an essential part of a *single* system of structures.

This unity emphasizes the community of function, or purpose, subserved by all the constituent tissues, and the host of adaptations between particular tissues, which so orderly a division of labor represents; each specialized function combines with all other specialized functions in rendering a single service to the organism as a whole. No one of them would have any utility, if isolated from the functions coördinated with it. It is as if each special part were willing to forego any credit which might come of rendering, unaided, a complete service to the organism. The mouth and the stomach, for example, are content to convert the food ingested into intermediate products, while to the small intestine, a not very dignified organ in popular estimation, is reserved the honor of converting these into final products. The work of the whole system, moreover, is merely one of preparation; metabolic activities proper begin only with the absorption of the food through the walls of the alimentary canal, into the blood or lymph. The digestive system there comes in contact with other systems which stand in the same relation to it as do parts of the digestive system itself to that system as a whole.

The structures involved in this system of activity are very specifically and precisely adapted to one another, since their several activities, with the resulting chemical products, must, as a final result, have prepared the food ingested, for the metabolic process of the organism, and in such form that these latter can, through its further elaboration, serve all the needs which food must ultimately supply. That modifications in one of these structures determining substantial changes, quantitative or qualitative, in the chemical substances produced, must be accompanied by such changes in other structures as will preserve the balance and serviceability of the entire system is too obvious to be em-

stuffs originally making up the meal, and in addition maltose, proteose, and peptone. The strongly alkaline bile and pancreatic juice quickly neutralize its acid and the various enzyms of the intestinal tract act upon it. The amylopsin of the pancreatic juice converts to maltose all starch not affected by ptyalin; the lipase of the same secretion splits the fats to fatty acid and glycerin; the trypsin of pancreatic juice, in coöperation with erepsin of the succus entericus reduces all proteins, including proteoses and peptones, to amino acids; the inverting enzyms, maltase, sucrase, and lactase, change all the double sugars, and therefore all the carbohydrates of the meal, to single sugars. The intestinal contents are churned and kept in onward progress by movements of segmentation and peristalsis performed by the muscular walls of the gut." Martin, H. N. and E. G., *The Human Body,* 10th edition, pp. 478–489; Henry Holt and Company, publishers.

phasized. The temporary breakdown of the digestive system from which every one has suffered, and which can often be traced to deviations from the normal activities of particular structures in the system, is conclusive evidence that the various structures and their activities must be precisely coadapted by heredity. Let us consider these facts on the interpretations supplied by anti-Lamarckian hypotheses, dealing first with their interpretation on the non-chemical form of the germinal hypothesis.

With the results of previous analyses in mind, the criticisms of that hypothesis, as applied to this case, may be very briefly indicated. (1) The hypothesis implies that the interadapted modifications in the structures of the digestive system that occur simultaneously are determined by simultaneous intra-germinal variations, and it holds, of course, that coöperative functional activity on the part of the structures themselves is not a factor in the genesis of those variations. To account for the fact that the simultaneous changes which occur have a *mutual reference* to one another, some sort of preëstablished harmony must be postulated, since, on the hypothesis, this mutual reference cannot be due to the interaction of the structures involved.

(2) If this postulate be rejected, the chances that these mutually adaptive changes should occur in the requisite number and order are infinitesimally small, according to implications of the hypothesis, as could be demonstrated by a repetition of our previous analysis. This conclusion is accentuated in the present case by the fact that a substantial change in one structure and its activity could not, as a rule, *wait* for the requisite changes in other structures and their activities, as an isolated substantial change in one structure and its activity would, generally speaking, so derange the system as a whole that the organism would quickly perish. Given one change of this character, simultaneous changes in a considerable number of structures and their activities would usually be necessary if the chemical equilibrium of the system were to be preserved. A germ-plasm with the variability requisite to the production of adaptive variations equal to these demands would be utterly unfit for survival.

(3) The hypothesis further implies that a modification in one of the constituent structures of the digestive system might *represent* other structures of the system, and without any sort of prior reaction to those structures. The proposition thus implied is a self-contradictory one, and otherwise repugnant to the basic postulates of modern science.

The quantitative conception of germinal changes obviously does not fit the facts of this case, since modifications of the type under consideration are qualitative in character. Modification of the structure deter-

mining the production of a chemical substance expressed by a formula different from that of the substance produced before is certainly qualitative in character, while a modification determining a difference in quantity of product is at least partially qualitative in character, since productive capacity could scarcely be regarded as altogether of a quantitative character, depending, as it must, on the general organization of the structure in question.

The chemical form of the germinal hypothesis seems at first sight to possess decided advantages over the non-chemical form of the hypothesis, as applied to an interpretation of the digestive system, seeing that the more significant activities of the system are distinctly chemical in character. But these advantages turn out, on examination, to be illusory, rather than genuine, in character. For, if the chemical changes assumed to be determinative of hereditary modifications in the digestive system all took place *within* the genes, factors or determinants operative in the development of that system, generation after generation, the previous analysis applies; and there are additional special difficulties such as have been opposed to this form of the hypothesis in its application to other cases.

If the chemical form of the hypothesis be made independent of the assumption that autonomous germinal units are operative in development, those special difficulties would be multiplied, and more than counterbalance any advantages derived from the rejection of that assumption. For the hypothesis would then have to account, in chemical terms, for the elaborate *organization* of the chemical processes in digestion, as well as of the complex constituent structures of the digestive system. It would also have to account for the fact that an adaptive equilibrium of the chemical processes is generally preserved, at least to the extent of assuring the survival of the species. There is nothing in chemical action which can account for the complicated organization and organizational changes thus indicated. This we shall hope to demonstrate in our discussion of the physicochemical theory of the organism, and of hereditary changes therein. That discussion may be taken as elaborating the summary treatment of the theory in relation to the present case.

Let us now consider the interpretation of the coadaptions within the digestive system, on the hypothesis of parallel induction. If applied to this case, the hypothesis would imply that substantially equivalent series of acquired and of hereditary modifications in the structure and functional activity of a given constitutent tissue in this system are induced by chemical or non-chemical action of other tissues, by elaborated or unelaborated food substances ingested by the organism,

or, perhaps, by environmental stimuli of other sorts, such as changes in temperature or humidity.

In considering the hypothesis thus applied, we need not attempt to decide which of the stimuli indicated might act concurrently on the given tissue and the corresponding germinal material, as located in the germ cells or in the given tissue itself. The available experimental data are not sufficient to justify any very confident answer to this question, granting the question itself to be a legitimate one.[1] It must suffice to point out the *implied* empirical consequences of the hypothesis in this connection. As in other cases, advocates of the hypothesis would not be able to account for the assumed equivalence between the series of modifications alleged to be induced by the same stimuli in the given tissue and its germinal determinants, respectively, without invoking some external agency capable of determining coördinations of this type. For the assumed independence of somatic and germinal activities in the same organism, the vast differences between the two systems of activity, and the complex series of changes intervening between germinal determinants as modified and the somatic tisues into which they develop would, taken together, necessitate a coördinating agency of this sort.

But scant consideration need be given the possibility of interpreting this case on the non-representative transmission hypothesis. A change in the amount or composition of a certain secretion, or in the routing or mechanical mixing of the food materials, would, in such a delicately organized system of activity, necessitate changes in other secretions or in other mixing or routing processes; and the non-representative transmission hypothesis would imply that specific hereditary readjustments necessitated by such changes are not produced by the corresponding somatic responses, save by chance, but by somatic responses corresponding to other specific readjustments. And that would require, as in other cases, a coördinating agency capable of effecting the assumed equivalence between the series of somatically induced responses and the series of functional modifications determined by heredity.

The exclusion of all possible anti-Lamarckian hypotheses as interpretations of the coadaptations embraced by the digestive system results in the reëstablishment of the Lamarckian hypothesis as applied to this group of characters. This hypothesis is supported both by the refutation of the opposing hypotheses, as applied to these characters,

[1] We can of course say that unelaborated food substances do not act "directly" on certain tissues and their germinal determinants; and that particular sorts of chemical or non-chemical action do not affect in this way all tissues and their determinants; but we cannot proceed from such negative statements to positive affirmations that specified sorts of stimuli do or might act on a given tissue and its determinants.

and by the strong inferential evidence that hereditary modifications of coadaptations in digestive processes are substantially equivalent to, if not preceded by, somatically induced modifications of the same coadaptations in the parental organisms. Possible qualifications of these conclusions and their vindication of the Lamarckian principle will be considered in our discussion of objections to the type of analysis here set forth, and in our systematic examination of physicochemical theories of vital phenomena.

We may remark, before proceeding to our next case, that the digestive system constitutes a complex active adaptation of the organism as a whole to specific features of its external environment, namely, the organic substances which the system is capable of elaborating for the uses of the organism. Our analysis of this adaptation, as interpreted on the Lamarckian and anti-Lamarckian hypotheses, would, however, yield results paralleling the results derived from our analysis of other active adaptations to the environment, as interpreted on these hypotheses. An enormous number of hereditary modifications, adapted to specific features of the external environment, are represented by the digestive system in man, and these imply an adaptive reference to that environment, and a representation of specific features thereof, as does, for example, the adaptation of the ladybird to the cottony cushion scales, or of an obligatory parasite to one species of host animal. The anti-Lamarckian interpretations of the digestive system, so regarded, would imply much the same empirical consequences as the corresponding interpretations of the adaptations just specified.

The same general considerations apply to the adaptations of connective tissues to the correlative tissues, and to the coadaptations embraced by the nervous system, presently to be considered. All these have a more or less explicit reference to specific features of the external environment, and a comprehensive analysis thereof would necessarily take that relationship into account. But the applicability of our former analysis to these cases is obvious, and need not be detailed in relation thereto.

Adaptations of the Nervous System

The third group of intra-organic adaptations to be employed as tests of Lamarckian and anti-Lamarckian hypotheses is presented by the nervous system of the higher animals, in which, of course, nervous processes are of primary significance, though these are associated with, and to some extent constituted by, physical and chemical processes.

The simplest functional unit of the nervous system is the so-called

reflex arc, which embraces at least three structures—a stimulus-receptor, an effector organ (gland or muscle cells), and a conductor between these two.[1] But the reflex arc is integrated into more complex nervous "mechanisms," and these, again, in endlessly complicated ways, into the nervous system as a whole, which acts as a single unit. Directly or indirectly, therefore, each of the constituent cells in this system is correlated (or coadapted) with all the other cells and they, in turn, with each cell. The degree of this interrelatedness is indicated by the fact that the nervous system in man, for example, is said to embrace some ten or eleven billions of nerve cells.[2]

Staggering is the complexity of the nervous system thus depicted, its complexity is raised to an even higher power when the detailed innervation of muscles and glands is taken into account. All the muscles and most of the glands are under the control of the nervous system, a control effected through the penetration of nerve-fibres into the innermost recesses of muscular and glandular tissue. The detail of muscular innervation is indicated by a standard treatise as follows: "after an efferent nerve has entered the substance of the so-called voluntary or striated muscle, it subdivides among the individual muscular fibres, separating these fibres from each other. . . . In the non-striated (or non-voluntary) muscles, the nerves divide and subdivide to

[1] Physiologists are disposed to regard the simple reflex arc as in the nature of an abstraction useful for purposes of analysis, but hardly if ever realized in the facts of nervous reaction.

[2] "The nervous system functions as a whole. Physiological and histological analysis finds it connected throughout its whole extent. Donaldson opens his description of it with the remark: 'A group of nerve-cells disconnected from the other nerve-tissues of the body, as muscles and glands are disconnected from each other, would be without physiological significance.' A reflex action, even in a 'spinal animal' where the solidarity of the nervous system has been so trenchantly mutilated, is always in fact a reaction conditioned not by one reflex-arc but by many. A reflex detached from the general nervous condition is hardly realizable.

". . . The afferent neurone on entering the central organ, the spinal cord, enters a vast network of conduction of paths interlacing in all directions. A glance at any Weigert preparation of the spinal cord shows a tangle of branching nerve-fibres, the richness and intricacy of which seems practically infinite. Into this forest the receptive neurone conducts the impulses, and can itself be traced, breaking up into many divisions that pass in many directions and to various distances. And this web of conductive channels into which the centripetal impulses of the reflex are thus launched is known to be practically a continuum in the sense that no part of the nervous system is isolated from the rest. . . . And there is the generally accredited statement that on exhibition of strychnine centripetal impulses poured in *via* any afferent nerve, excite reflex-discharge over the efferent channels of the whole nerve-system. This, even if not strictly true, is sufficiently approximate to the truth to show the enormous interconnections between any afferent channel and the congeries of arcs of the whole central nervous system." Sherrington, C. S., *The Integrative Action of the Nervous System,* pp. 114–115, 152–154; Yale University Press, publishers.

form more and more minute plexuses of nerve-fibres, which are distributed in the connective tissue that separates the muscular fibres from each other, and finally applied to the surfaces of the muscular fibres." [1]

We should bear in mind, in considering the innervation of the muscles, that "each muscle-fibre is developed from a single cell and so constitutes a single histological element," so that there is a hereditary connection between the individual cells of muscular tissue and *processes* of individual neurons—that is, between single cells on the one side, and definite cell-parts on the other. The enormous number of such connections is indicated by the fact that the fibres of striated or voluntary muscle, while varying greatly in size, range "in length from 1 up to 35 mm. ($\frac{1}{25}$ in. to 1 $\frac{1}{3}$ in.), and in diameter from 0.034 to 0.055 mm. ($\frac{1}{750}$ to $^{1}/_{450}$ in.);" and that the non-striated (or non-voluntary) muscle cells "on the average are about $\frac{1}{24}$ mm. ($\frac{1}{600}$ in.) in length." [2]

The detail of glandular innervation has not been worked out completely, but it is known that nerve-fibres penetrate at least as far as the lobes and lobules of the gland, and there is reason to believe that they "become directly united with the secreting cells of some glands."

Finally, we must remember that the striated muscles are for the most part attached to the skeleton and constitute with it and the coördinated nerves a single system; while the non-striated muscles "surround cavities or tubes in the body, as the blood-vessels and the alimentary canal," likewise constituting with these organs and the coördinated nerves one system.

Our analysis of connective-tissue adaptations applies, with modifications, to the present group of adaptations, and we shall accordingly adapt the results of that analysis, in a summary fashion, to these cases. We shall begin, as usual, with the interpretation yielded by the non-chemical form of the germinal hypothesis.

To start with the simple reflex arc, we have at least three separate organs or structures—the stimulus-receptor, the conductor and the effector organ—precisely coadapted by heredity. Substantial variations in any one of these structures must be accompanied by variations in one or more of the coördinated structures. But since the simple reflex arc itself does not exist in isolation, being related, by way of more complex mechanisms, to the nervous system as a whole, we may say that each of the neurons constituting the nervous system is adapted indirectly to all the other neurons, or at least to systems

[1] Ladd, G. T. and Woodworth, R. S., *Elements of Physiological Psychology*, p. 212; Charles Scribner's Sons, publishers.

[2] Martin, *The Human Body*, 10th edition, 1917, pp. 83, 86; Henry Holt and Company, publishers.

into which they are organized. Moreover, the unit of nerve adaptation to effector organs is not the neuron, but the nerve-fibre, a neuron process, each of the efferent neurons thus serving a number of gland or muscle cells. The unit of adaptations on the side of the effector organs, is a single cell in the case of the muscles, and a cell or group of cells in the case of the glands. All these adaptations must be regarded as hereditary, for they develop generation after generation according to their specific types and, as we may assume, in substantially the same numerical proportions. We then have an all but infinite number of receptor, nerve, gland and muscle cells varying with reference to one another, not to mention their more remote reference, in the process of variation, to skeleton, connective tissue and visceral organs, all of which are interdependent with the specific classes of structures here under consideration.

The interpretation of these facts on the germinal hypothesis involves consequences which emphasize difficulties of the sort opposed to that hypothesis in relation to other cases. For it gives us the picture of billions on billions of hereditary variations occurring, which are adapted to, and therefore represent, one another, but without the interaction of the structures involved being a factor in the process. The only way in which adaptive variations of this order can be accounted for on the germinal hypothesis is to couple it with the assumption of a preëstablished harmony between the variations in question. Without that, a germ-plasm must be assumed which is utterly unfit for survival on account of the infinite variability essential to its generation of adaptive variations in the requisite measure and order. Even this assumption, were its validity granted, could not reconcile us to the further implication of the hypothesis, that the variations in all these structures *represent* variations in the coördinate structures—which are unique events in nature—without any prior reaction thereto. On this showing, to repeat conclusions yielded by previous analyses, the germinal hypothesis is a mass of contradictions, a verbal formula to which no determinate content whatsoever can be assigned.

The quantitative conception of germinal changes could not serve to mitigate these consequences of the hypothesis, since the adaptations of this group, from the lowest order of complexity to the highest, are of a specific and qualitative character. That conception has therefore no language whereby the facts under consideration may be described, and it could not therefore be employed in an analysis of the germinal hypothesis as applied to these facts.

The chemical form of the hypothesis labors under the same disadvantages in this as in other cases already considered. If associated

with the assumption of autonomous germinal units, as operative in the development and evolution of the structures in question, it meets with the identical difficulties that beset the non-chemical form of the hypothesis, besides the special difficulties of accounting for the organization represented by these structures and by the orderly changes therein. If dissociated from the assumption of autonomous units, its special difficulties are so multiplied that any advantages derived from that dissociation are more than counterbalanced.[1]

We may now examine the interpretation of this group of adaptations on the parallel induction hypothesis. To simplify the analysis, we shall consider only the adaptations of single neurons to other neurons, to muscle or gland cells, and to receptor organs. The environmental stimuli which might be conceived to induce equivalent adaptations in neurons, including their processes, and in germinal determinants of the given neurones (which determinants, be it remembered, may be situated *in* the corresponding somatic parts) would perhaps include changes in size, weight or shape of the structures *to* which the given adaptation has reference; changes in the chemical activity, electrical phenomena or other physical reactions of those structures; or external stimuli affecting the organism as a whole, such as changes in light, temperature, moisture or food substances.

As in previous cases, we have no means of deciding whether or to what extent any such stimuli could affect "directly" the nerve cells and their germinal determinants. We may say, indeed, that moisture or unelaborated food substances could scarcely affect nerve cells and their determinants directly, except, perhaps, nerve cells partially or wholly constituting various stimulus-receptors; for the organism assimilates or reacts to those stimuli before they could reach any of the nerve cells save those mentioned. And we may say, in view of the discussion of connective-tissue adaptations, that changes in the size, shape or weight of the structure to which nerve cells are adapted could not modify the latter directly, but would have to set up some sort of functional stimulus before they could have any effect on those cells.

It is unnecessary for our purposes to speculate as to the possible "direct" effects of environmental stimuli other than these, on nerve cells and their germinal determinants. As in other cases, the implied empirical consequences of the hypothesis invoking such alleged effects of environmental stimuli will enable us to evaluate that hypothesis. As in those cases, the equivalence of the assumed somatic and germinal

[1] G. Wolff has criticised in much the same way as we have here, the selectionist interpretation of coadaptations between nerves and muscles. See his *Beiträge zur Kritik der Darwins'chen Lehre,* 1898, pp. 8 ff. A brief reference to this criticism will be found in Kellogg, V. L., *Darwinism To-day,* pp. 51–52.

modifications induced concurrently by the same stimuli cannot be accounted for, on that hypothesis, without adding to it the assumption of some external agency capable of determining coördinations of this type. These difficulties of the hypothesis are accentuated when the all but infinite number of adaptations denoted by the nervous system are taken into consideration. All this on the supposition that the parallel induction hypothesis might be applied to this group of adaptations.

We shall not repeat for the possible interpretation of these adaptations on the non-representative transmission hypothesis, the analysis to which that hypothesis has been subjected in relation to other cases. As in those cases, an external coördinating agency would of necessity be brought into play to account for the assumed equivalence of the somatically induced modifications and the hereditary modifications in later generations, as determined by the somatic modifications, but not in a representative sense.

The exclusion of the several anti-Lamarckian hypotheses as interpretations of these adaptations results in the reëstablishment of the Lamarckian hypothesis as applied thereto. The hypothesis is supported by the inferential evidence derived from a comparison of the empirical consequences implied by these several hypotheses, and in a positive sense by the analogical evidence found in the similarity of learned responses, or acquired adaptations, to unlearned responses, or hereditary adaptations. To take a single illustration of the latter, the young chick's unlearned reactions to spatial perceptions are strikingly similar to learned responses correlated with perceptions of the same class. Evidence of this kind could be cited indefinitely, and, when exempted from anti-Lamarckian interpretations, positively supports the Lamarckian hypothesis. There is no available evidence of this sort relative to the genesis of coadaptations such as subsist between nerve-fibres and glandular tissues, but the inferential evidence bearing on this process goes to support the Lamarckian interpretation thereof.

As in other cases, this affirmative conclusion is conditional, for its final validity, on the outcome of our subsequent examination of the various objections which may be urged against the assumptions, methods and conclusions of the analysis herein set forth.

Intra-cellular Adaptations

We do not know enough about the structure and functions of the cell to make so graphic an application of our analysis to intra-cellular adaptations. But if the cell is a real organism with definitely coördinated parts and functions, and if cell structures and functions are as

truly the products of evolutionary processes as are other structures and functions of the organism, then our general analysis will apply to them equally well. We must conclude that adaptations of cell-part to cell-part and to the cell as a whole have been evolved through the coöperative functioning of these structures, and, consequently, that they have originated neither as independent variations in an isolated germ-plasm, whether conceived in chemical or non-chemical terms, nor as non-representative effects of functional activity, nor, again, as the products of direct action by environmental stimuli. These latter hypotheses would, in application to this group of phenomena, land us in much the same difficulties as we have opposed to them in other applications.

Our analysis of combinations among anti-Lamarckian hypotheses, in relation to adaptations of the organism as a whole to its environment, will apply, without any particular qualifications, to such combinations when employed in the interpretation of intra-organic adaptations. It was there shown that the same criticisms apply to these hypotheses, in whatever combination they may be applied to active adaptations, as to the constituent hypotheses, when applied separately to these same adaptations. It was pointed out that such combinations would also suffer from inconsistencies and obscurities resulting from the attempt to synthetize more or less incompatible hypotheses; and that any such combination would rest on the same artificial divisions of the organism into independent systems of vital activity, as do the hypotheses which would enter therein. As aforesaid, the same criticisms apply to interpretations, on such combinations, of intra-organic adaptations.

Applications of the Analysis of Hereditary Active Adaptations as Interpreted on Lamarckian and Anti-Lamarckian Hypotheses

The analysis in this and the preceding chapter has employed typical active adaptations to environment as tests of Lamarckian and anti-Lamarckian hypotheses respecting the origin of hereditary variations. Before entering upon the consideration of objections to this analysis, the scope of its application must be briefly indicated.

The analysis applies, we believe, to *all* active adaptations to the environment, whether of the cell-part, cell, tissue, organ, organ-system or organism as a whole. Such adaptations, in the sense of the term employed in the present discussion, are coterminous, in their distribution, with life itself, since life does not exist apart from determinate systems

of structures and functions having explicit reference to more or less specific features of the environment. The category includes hereditary adaptations to features of the external world constituting the medium, to organic or inorganic bodies serving as food substances or coöperating in the reproductive processes of the organism, to the objects and relationships of three-dimensional space, to features of the environment constituting risks or dangers to the organism, as a predatory species or an obligatory parasite, to organisms entering into symbiotic or commensal relationships with the given organism, and, on the part of constituent structures and functions of the organism, to other specific structures and functions which, together with the former, constitute a single system of vital activity.

Our analysis could be repeated for all such adaptations and would lead, with the necessary qualifications, to similar results. An inherited active adaptation of any description whatsoever implies that the germ-plasm has been so modified as to have reference to certain features of the environment, rather than to other features to which adaptation was theoretically possible. Such an adaptation implies, moreover, that the germ-plasm represents, in a genuine sense, the specific features of the environment to which it has reference.

No such adaptation can be accounted for, on the germinal hypothesis, except by combining therewith the assumption of a preëstablished harmony between the variations of the germ-plasm and the specific features of its environment; or else the assumption of a germinal variability so nearly infinite in scope as utterly to unfit the organism for survival. And neither assumption could render intelligible the fact that hereditary adaptations *represent* specific features of the environment and, in a large proportion of cases, unique and irreproducible features thereof.

The apparent advantages enjoyed by the chemical form of the hypothesis, where the chemical conditions render the interpretation of adaptations on that hypothesis more plausible, turn out, on examination, to be largely illusory, and, even where genuine, to be counterbalanced by special difficulties encountered by this hypothesis. An organic adaptation is essentially a *non-chemical* phenomenon, not susceptible of translation into chemical terms; and the structure or structures constituting one or both terms of the adaptation are likewise non-chemical phenomena, for which chemistry has no interpretation and, indeed, no language. This position will be established, we venture to hope, by our analysis of physicochemical conceptions of life.

Likewise, the quantitative conception of germinal changes has no language for the facts here in question, as active adaptations to en-

vironment are of a qualitative nature, though there may be quantitative measures of the stimuli evoking various responses constituting such adaptations, and even of qualitatively similar responses to the same stimulus by different species, or by different members of the same species, or by the same organisms at different times or under different conditions (other than the stimulus in question). These quantitative features of active adaptations have not figured in our analysis, for we have employed as tests of the hypotheses under examination, distinguishable types of active adaptations which are incommensurable with one another. And it is doubtful whether genetic interpretations of these quantitative aspects of active adaptations could themselves be expressed in quantitative terms, but, even if they could, the fact would not necessitate any qualification of our analysis, within the limits which we have imposed upon it.

The interpretation of *any* active adaptation on the hypothesis of parallel induction implies that the given environmental stimulus produced equivalent modifications of functional activity in parent and offspring, by acting on *wholly different* structures—the soma in the one case, and the germ-plasm in the other. Such equivalence of modifications could not be accounted for, on this hypothesis, except by combining with it the assumption of a coördinating agency *ab extra,* or at least an agency with whose mode of operation we have no acquaintance.

It should be recalled in this connection that none of the experimental evidence bearing on the question demonstrates that parallel induction occurs as a matter of fact. For the most cogent of this evidence, the reported results of Tower's experiments on Leptinotarsa, has been called in question; and, even were it taken at its face value, its interpretation on the hypothesis of parallel induction is open to doubt, as Semon has shown.[1] Even should the parallel-inductionist interpretation of these and similar cases prove successful, it would not affect the validity of our analysis, as the hereditary variations involved are not active adaptations in our sense of the term. We recognize, of course, that chemical substances and other sorts of environmental stimuli may and do affect the germ-cells, either "directly" or by way of the soma. It is the parallel induction of equivalent modifications, particularly active adaptations, in soma and germ-plasm that we question. Let us recall, finally, that the environmental stimuli concerned in the genesis of many active adaptations are not capable of affecting the corresponding germinal determinants "directly."

The interpretation of *any* active adaptation on the non-representative

[1] *Supra, p.* 35.

transmission hypothesis implies that some external coördinating agency was operative in determining the equivalence between the hereditary adaptation and the acquired adaptations in the ancestral organisms. The only possible qualification of this conclusion would be that, while functional activity does not normally play a representative rôle in the genesis of active adaptations, it might do so by chance. The probability of a representative hereditary modification being produced by such activity is, by hypothesis, the same as that of any non-representative modification which functional activity of the given sort may be supposed capable of producing. We saw, however, that such probability of representative modifications being so produced could not account for the genesis of adaptations, since such chance representative modifications would not occur in the measure and order necessary to the establishment of the active adaptations exhibited by the organism, and especially of complex adaptations including a number of distinguishable functional processes. Moreover, if we may regard the adaptations herein interpreted on this hypothesis as typical, functional activity could not play a non-representative rôle in many adaptations, since the functional activities involved are so circumscribed, as in the case of connective-tissue cells, that a representative rôle must be attributed to such activity, if functional activity is to be considered an active factor in the genesis of such adaptations.

It is perhaps unnecessary to qualify, so as to apply to all active adaptations, the interpretation of our cases on various combinations of the anti-Lamarckian hypotheses. An interpretation of any such adaptation on two or more of these hypotheses combined would doubtless involve the same consequences as its interpretation on the constituent hypotheses of such a combination taken separately. Other objections urged against such combinations—their incompatiblity, their several assumptions of independent systems of activity within the organism, etc.—would apply in all cases to which such combined hypotheses might be applied.

If our criticism of anti-Lamarckian interpretations of active adaptations employed in the foregoing analysis applies, with necessary qualifications, to interpretations of any active adaptation on anti-Lamarckian principles, then we are justified in concluding that all such adaptations must be interpreted on the Lamarckian principle, in the modified form here proposed. That interpretation is supported in every case by the exclusion, as interpretations, of all alternative hypotheses, as also by empirical evidence, direct in some cases, and indirect or inferential in other cases.

If we may take the cases employed by us as typical, the empirical evi-

dence supporting the Lamarckian hypothesis is at least as good in every case as evidence supporting the rival hypotheses, while far superior to it for many types of active adaptations. More decisive is the comparison of the empirical consequences implied by the Lamarckian and anti-Lamarckian hypotheses, respectively. All the anti-Lamarckian hypotheses without exception assume phylogenetic processes in the organism for which there is no empirical evidence, or at least no adequate empirical evidence; and each of them must be combined with some sort of preëstablished harmony or external coördinating agency if it is to account for the correlations between hereditary adaptations, on the one side, and environmental conditions or functional activities, on the other side. Even this added assumption would not avail to render intelligible any germinal interpretation of active adaptations. For any form of that hypothesis would still imply that active adaptations represent specific, often unique and irreproducible, features of the environment, without prior reaction thereto. Ability to represent something without that something or its equivalent having ever been *presented* to that which represents it, or involved in the establishment of such representation, is akin to the creation of something from nothing. It is, indeed, a contradiction in terms. Science has no place for any such conception. The Lamarckian hypothesis, on the other hand, involves no insupportable consequences of this character.

To prepare the way for a registration and examination of objections to our analysis, some additional specific applications thereof may be indicated. We shall largely limit ourselves, in this connection, to types of adaptations which appear to offer the most serious difficulties to the analysis.

To begin with, we venture to assert that our analysis, with its vindication of a modified Lamarckian interpretation of hereditary adaptations, applies to all types of instinctive behavior in animals, even to the most complex instances thereof, which have reference to more or less specific features of the environment. It applies, for example, to two cases often cited, and later considered, namely, the instinctive activities of the Yucca moth (*Pronuba yuccasella*) in connection with oviposition and provision for the young; and the complex reproductive activities of a beetle of the genus *Sitaris,* which are bound up with certain behaviors of the mason bee *Anthophora.* It similarly applies to all other complex instinctive behavior in insects, so far as this falls within our category of specific adaptations to the environment.

The analysis applies also to the interrelationships within complex structures and functions, such as those of the vertebrate retina, for example, which have shown themselves refractory hitherto to any sort

of Lamarckian interpretation, and have been deemed, for that reason, to supply specially cogent evidence in support of the germinal (or selectionist) hypothesis respecting the origin of variations. We shall consider adaptations of this type which may be deemed to present serious difficulties to our analysis.

We are not implying that the Lamarckian principle could alone supply an adequate account of adaptations such as these, or, indeed, of any active adaptation. Our contention is that any satisfactory account must allow for the Lamarckian principle, but *also* for other agencies or processes, as operating in conjunction with it. To anticipate our analysis of complex instinctive activities in the lower animals, we shall find grounds for the assumption that organizatory factors are involved in the evolution of such activities. But such organizatory factors taken alone cannot account for phenomena of this type. Likewise, physicochemical factors both within and without the organism, and features of the environment not translatable into physical or chemical terms, are involved in the genesis of active adaptations, but cannot, as we expect to show, entirely account for characters of this sort.

Our general analysis does *not* apply, save indirectly, to passive adaptations, to characters possessed of no demonstrable adaptive value, or to physicochemical processes within the organism, such as chemical reactions in the digestive system, when these characters are considered in abstraction from the vital *organization* to which they are all subjected. Nor does the analysis apply to vital factors considered in abstraction from the bodily processes of all kinds which are organized by these agencies. Under the latter category would come those general properties of living organisms which apparently antedate any evolution of life. We refer to the necessary dependence of life on matter and energy, the processes of metabolism, the capacity for growth by intus-susception, the property of reproduction, and the capacity for adaptation to the environment. The specialization of these general properties, however, is of the very essence of evolution, and necessarily involves increasingly specific adaptations between the constituent functions and structures of the organism, and of the organism as a whole to its environment. Such specializations in the general properties of life would therefore come within the scope of our analysis.

Nor is the analysis devoid of any application to the genetic interpretation of passive adaptations or of characters having no demonstrable adaptive value. For if characters of these groups result from changes within the organism, functional activity is necessarily involved in the genesis thereof, and functional activity cannot be dissociated into two

species of activity, one of which has an adaptive reference to other processes of the body, and one of which has no such reference. Apparently, however, functional activity does not, in these cases, have a representative influence on hereditary characters, but contributes its *products,* so to speak, to their genesis. Just as, in the case of complex instinctive activities in the lower animals, genetic theory must apparently lean heavily on the assumption that morphogenetic factors are operative in phylogenesis; so, likewise, genetic interpretations of passive adaptations and non-adaptive characters must apparently give a good deal of weight to the same class of factors, and even more weight, in some cases, to physicochemical factors.

But these qualifications and concessions do not invalidate the theory that functional activity is a phylogenetic factor of universal scope, since, on the analysis, active adaptations are of the very essence of vital activities, and these cannot be accounted for without imputing to functional activity a representative rôle in their genesis. While the physicochemical and morphogenetic factors *always* associated with functional activity are also involved in the genesis of all organic characters, they merely find in the classes of characters under consideration, a specially striking exemplification.

CHAPTER V

CRUCIAL PROBLEMS FOR THE ANALYSIS OF LAMARCKIAN AND ANTI-LAMARCKIAN HYPOTHESES

Objections to the analysis of Lamarckian and anti-Lamarckian hypotheses set forth in the preceding chapters might be drawn from the following sources.

(1) The *non-occurrence* of hereditary modifications which might have been produced by functional activity, in conjunction with other factors. Under this heading would come the non-inheritance of mutilations, the non-transmissibility of most diseases, the failure of races or species to acquire hereditary immunity to particular infectious diseases after long exposure thereto, the negative results of transplanting ovaries from one female to another, or of grafting together parts of different organisms, the clear segregation in the germ cells, of heterozygous factors, though combined together generation after generation, the non-inheritance of visual images or other effects of sensory experiences, the apparent failure of learning to influence hereditary characters, and so on.

(2) The existence of active adaptations, in the genesis of which functional activity was apparently excluded altogether, or on which it seemingly could not have exerted a representative influence. Without being too meticulous at this point regarding the proper classification of certain characters which might be supposed to come within this category, we may put down as instances of this class the evolution of flowering plants in such a way as to assure fertilization through the visits of insects; the extension of the lung sacs in birds, through very fine perforations, into the bones; the restitution of mutilated parts and allied phenomena; the orientation of animals to a galvanic current, when subjected thereto, though they never reacted to such a stimulus in their natural environment; the positive heliotropism of animals which do not live in the light, and so on. In this category would also come many complex instinctive activities of the lower animals, and complicated adaptive structures found in most groups of multicellular animals. But these are so crucial for our analysis as to merit special consideration.

(3) Complex instinctive activities, already referred to. These are illustrated by the reproductive behaviors of *Sitaris* and of the Yucca moth, as well as many other insects. In these cases many of the acts included in the instinctive behavior series occur but once in a lifetime, and in some cases the performer never experiences the adaptive result of its behavior. In this general category would come also the instincts of neuter insects.

(4) Complex structures having a high adaptive value, such as the retina and other complicated receptor organs. According to the present way of viewing the problem, *all* complicated inner structures would come within this category of characters deemed to be refractory to a Lamarckian interpretation. Lamarckians have hitherto not been very convincing in their attempt to account for this group of characters.

(5) More extreme *Lamarckian* interpretations of hereditary variations than are favored by our analysis; or, not to prejudge, by an adjective, the issues involved, interpretations which emphasize more than we do certain features of the Lamarckian theory. Psycholamarckism, or psychovitalism, as propounded by Pauly and others, is the type of doctrine here in question. Other versions of Lamarckism are criticised herein indirectly and incidentally, rather than explicitly and systematically.

(6) The "inconceivability" of a transmissionist mechanism, or at least failure hitherto to demonstrate the existence of such a mechanism. This objection has already been considered, incidentally, in connection with our general analysis, but a more detailed examination thereof will not be out of place.

(7) The methods and underlying asumptions of the analysis. Our more positive analysis has proceeded from assumptions tacitly accepted as valid, as against assumptions underlying genetic theories combated by the analysis. Although certain of these assumptions have been summarily treated in connection with the foregoing analysis, they must all be examined with reference to specific criticisms which may be directed against them, and the competing claims of assumptions which may be opposed to them more fully considered. Also, the methods employed in the analysis are likely to come in for specially drastic criticisms from certain quarters, and these criticisms must be faced and their value determined.

The more serious objections to our analysis, as drawn from these various sources, will be considered at length, in the order indicated.

(1) Failures of Functional Activity to Induce Hereditary Modifications

The first group of objections to our analysis and its vindication of the Lamarckian principle have reference to the *non-occurrence* of hereditary modifications which functional activity in conjunction with other factors might have produced.

These objections do not seem to us very weighty, for reasons to be stated presently; and, even if they were, no final answer can be returned to many of them, owing to the incompleteness of our knowledge regarding the factors in phylogenesis, and the limitations within which they operate.

It might be a sufficient general answer to all these objections, should we concede at once that no *Allmacht* is claimed for functional activity as a factor in phylogenesis, and assume that the *non-occurrence* of hereditary modifications which functional activity might produce, in conjunction with other factors, merely illustrates the limitations of its influence, besides performing the more useful service of indicating the nature of those limitations. Other factors share with functional activity the blame for not producing the modifications which hypercritical biologists demand of it, if its claims are to be allowed; for other factors are always associated with functional activity in whatever it undertakes to do, and should properly come in, therefore, for much the same sort of praise and blame as may be meted out to the latter for its failures and successes. Moreover, if we shall be able successfully to defend our position (not challenged by the present group of objections) that functional activity is operative in the genesis of characters coterminous, in their distribution, with life itself, that is about all which could reasonably be demanded of an analysis purporting to demonstrate that functional activity is *one* of the primary factors in organic evolution. Perhaps these general observations will justify us in passing rather lightly over the group of objections now under consideration.

Of these objections, let us deal first with the non-inheritance of mutilations, considered as evidence against the Lamarckian hypothesis.

It is significant, perhaps, that the evidence from mutilations, whatever its character, should now be generally conceded to have no particular significance for the questions at issue between Lamarckians and their opponents.[1]

Since, however, the evidence from mutilations still figures in the discussion of phylogenesis, we may indicate briefly some of the current

[1] Cf. Thomson, J. A., *Heredity,* 2d ed., p. 223.

views regarding the matter. The attitude of anti-Lamarckians seems to be merely one of satisfaction that little or no authentic evidence in support of Lamarckian views can be drawn from this source, whereas the Lamarckians themselves reluctantly admit that this is the case, although one Lamarckian writer has attempted recently to show that some good evidence *can be* drawn from this source.[1]

It has been pointed out by various writers that a traumatic defect is not a reaction of the organism to a stimulus, but is itself a stimulus which may provoke various types of compensatory reactions.[2] In attempting to rehabilitate some of the evidence from this general source, Semon contends that the reactions of the organism to traumatic injuries result, in some cases, in hereditary modifications of structure, while (to account for seemingly negative instances) the compensatory reactions, in a vast majority of cases, take the form of regeneration (in the animal kingdom) or of budding (in the plant kingdom).[3]

As evidence for the view that compensatory reactions to such stimuli sometimes modify hereditary structures, Semon cites the results of Blaringhem's experiments on maize and other plants, Klebs' experiments on *Sempervivum acuminatum*, and Kammerer's experiments on *Ascidie Ciona intestinalis*.

Blaringhem subjected his plants to various mutilations, thereby inducing numerous modifications, some of which were inherited. In certain cases, new varieties were produced, which apparently bred true generation after generation.[4] It has been objected to these experiments that Blaringhem was not working with pure-lines, and that his results are therefore of questionable value.[5] Semon rebuts this objection by pointing out that even if the modifications induced by the given stimuli were latent in the stocks experimented with, these were strengthened by the compensatory reactions to those stimuli.[6] We may reserve our own comments on this case until other experiments with mutilations have been cited.

Somewhat similar are Klebs' experiments on *Sempervivum acuminatum*, in which excessive nourishment and abnormally high tem-

[1] Semon, R., *Vererbung "erworbener Eigenschaften,"* Chap. V.

[2] Semon, *op. cit.*, p. 52; Rignano, E., *Inheritance of Acquired Characters*, p. 170.

[3] *Loc. cit.*

[4] Blaringhem, L., "Mutation et Traumatismes," *Bull. scientifique de la France et de la Belgique*, 1907; "Production d'une variété d'Épinards Spinacia oleracea, var. polygama," *Comptes Rendus Hebd. des Séances de L'Académie des Sciences* (Paris), Vol. 147, 1908, pp. 1331–1333; "Production par traumatisme d'une forme nouvelle de maïs à feuilles crispées," *ibid.*, Vol. 152, 1911. Summarized in Semon, *op. cit.*, pp. 60–61.

[5] Bateson, W., *Problems of Genetics*, p. 230.

[6] *Loc. cit.*

peratures, as well as mutilations, were employed as stimuli. These stimuli induced many abnormalities in the blooms, petals, etc., some of which were inherited.[1] These experiments are not very decisive, as divers stimuli were employed and the possibility was not excluded that some of the results reported were due to crossing. Klebs himself has been sceptical as to the value of these results, though maintaining that the modifications induced were, in some cases, transmitted for at least one generation.[2]

Another instance of positive results from experiments with mutilations was reported by Kammerer, who cut off both siphons of an ascidian (*Ascidie Ciona intestinalis*) and found that both organs were regenerated, *but with abnormal length,* and that this peculiarity was transmitted to offspring. "Es liegt hier also," writes Semon, "die Vererbung einer typischen Antowortreaktion auf die Verstümmelung in durchaus spezifischer Form vor." [3] Other biologists have been extremely sceptical regarding the authenticity of the reported results from this experiment,[4] and no very great value can be ascribed thereto unless they should be confirmed by other experimenters.

We have cited only some of the experiments expressly designed to determine the influence of mutilations on hereditary characters, and shall pass by the many cases of mutilations alleged to be inherited by advocates of the Lamarckian hypothesis. These figure rather conspicuously in many of the popular treatises on biological questions, but, as already stated, they are conceded to have no very great value as evidence, one way or the other. The experiments cited seem at least to leave the possibility open that compensatory reactions to traumatic injuries may sometimes lead to hereditary modifications of structure, in the Lamarckian sense.

Plausible reasons could be assigned for the non-inheritance of most mutilations, whether occurring accidentally or repeated generation after generation. It could be claimed, for example, that the amputation of mice's tails for twenty generations, the circumcision of male infants practiced by the Jews or the rupture of the hymen in the first sexual intercourse has no effect on hereditary structure, simply because these mutilations are not of such a nature as to provoke any special compensatory reactions on the part of the organism. A plausible explanation of why accidental injuries never have any effect on the

[1] "Über die Nachkommen künstlich veränderter Blüten von Sempervivum," *Sitzungsber. d. Heidelberger Akad. d. Wiss., math-naturw.* Kl., 5. Abhandl., Jahrg. 1909. See Semon, pp. 59–60, 162.

[2] See Klebs' letter to Bateson, *op. cit.,* pp. 211–212.

[3] *Op. cit.,* p. 59.

[4] Jennings, H. S., *Life and Death,* etc., p. 203.

next generation (if they never do!) could be found in the stability of hereditary structure and its imperviousness to the influence of functional modifications occasionally induced by mutilations. Again, mutilations are often of such a nature that the compensatory reactions thereto could have no visible effect on the offspring. The loss of an eye or an arm, for example, is a stimulus, the compensatory reaction to which is a greater use of the other eye or arm, and we could hardly expect a similar reaction to show up in any striking fashion in the offspring.

But a more inclusive statement of the logic in the case would be to say that we can find, from the standpoint either of the Lamarckian or of any anti-Lamarckian hypothesis, plausible explanations for either the inheritance or the non-inheritance of mutilations. Which amounts to saying that we do not really know why mutilations are not inherited, if they are not, or why they are inherited, if they are! The Lamarckian hypothesis, as entertained here, is in no wise embarrassed by the rarity with which mutilations are inherited, if they are ever inherited at all, or even by the rarity with which compensatory reactions to mutilations determine hereditary modifications in offspring, if they ever do determine such modifications. The hypothesis would of course derive additional support from a demonstration that mutilations or compensatory reactions thereto are sometimes inherited, but it can get along very well without such support.

The inheritance or non-inheritance of mutilations does not affect, so far as we can see, the validity of our analysis of hereditary adaptations to the environment, taken as tests of Lamarckian and anti-Lamarckian hypotheses. That analysis purported to show that a large class of hereditary modifications *which have actually occurred* can be accounted for only on the hypothesis that functional activity (always in conjunction with other factors) played a representative rôle in their genesis. This massive evidence from what has occurred is more conclusive, to our way of thinking, than is evidence from what has not occurred, especially since the latter, in the present case, can be interpreted passably well from the standpoint of any hypothesis concerned in the case.

Much the same general considerations apply to diseases and the hereditary characters involved therein. We know so little about the subject, generally speaking, that no very decisive evidence for or against any hypothesis respecting the origin of variations can be drawn from a consideration of it. We shall be justified therefore in dismissing this subject with only a very summary treatment thereof.

There is general agreement that *predispositions* to certain diseases

may be inherited, as, for example, tuberculosis, gout, rheumatism and certain nervous diseases included within the general category of insanity. It is also agreed that various degrees of immunity to certain diseases—the term usually being restricted to diseases produced by parasitic organisms—may be inherited by different races, or by different family stocks in the same race. It is generally assumed, furthermore, that somatic or developmental conditions coöperate in the production of various diseases to which there is a hereditary predisposition, although the hereditary factor may be so potent that the disease in question is likely to occur though there be no somatic or developmental conditions specially favoring its occurrence.

The difficulties of interpreting the facts in such cases arise not only from the obscurity of the conditions involved, but also from the fact that chemical processes, functional activity and organizatory factors all coöperate in the genesis of the hereditary characters concerned in all such cases. A further complication arises from the fact that, when considered from the Lamarckian standpoint, many or most diseases might have either one of two opposite effects on the hereditary constitution of the organism. First, the deleterious effects of the given disease on the organism might exert a representative influence on the offspring, and produce or accentuate a predisposition therein to the same disease. Second, the defensive reactions of the organism to the disease in question might strengthen in the offspring the factors or capacities making such defensive reactions possible. The effect on offspring of tuberculosis in the parent, for example, might be to accentuate or induce a predisposition to the disease, or, instead, to strengthen the hereditary capacity for "resistance" to the disease. There would, of course, be neither of these effects on offspring where the disease was fatal or incapacitated the individual for reproduction.

These considerations will suffice to show that no very decisive evidence for or against the Lamarckian hypothesis is supplied by the facts of pathology, at least not through the usual methods of dealing with such evidence. There is one type of evidence, however, which will yield more positive results, if handled by a different method. Many facts from this source can be brought within our category of hereditary active adaptations to specific features of the environment, and to these the analysis set forth in relation to such adaptations will apply, with similar conclusions as to Lamarckian and anti-Lamarckian interpretations thereof. We mean hereditary immunities to the attacks of pathogenic organisms, as manifested by particular races or by particular stocks of the same race. Even this evidence, when treated by the methods of our analysis, does not yield absolutely conclusive results.

Varying degrees of immunity to particular infectious diseases, as manifested by different individuals and races, are susceptible of two different interpretations. One is that such immunity or degree of immunity may be the result of defensive reactions to the pathogenic organisms in question, by the ancestors of the races or individuals possessing this immunity. The other interpretation is that those races or individuals became immune to the attacks of the given pathogenic organisms, quite apart from any defensive reactions thereto on the part of their ancestors, and in ways as yet quite unknown to us.

The first type of interpretation may be credited, in view of our previous analysis, to the Lamarckian hypothesis. Of course morphological and physicochemical factors coöperated with functional activity in the genesis of the immunities in question, if they really come within the category of specific hereditary adaptations to the environment. We must, however, consider the question whether they can be so classified. This brings us to the second type of interpretation.

The latter must be clearly distinguished from the germinal (or selectionist) interpretation of immunity. The germinal interpretation would assume that immunity of a race or stock to a specific infectious disease is a hereditary adaptation to a specific feature of the environment, the particular pathogenic organism in question. But the germinal interpretation of immunity thus regarded involves consequences similar to those shown to be implied by the germinal interpretation of parasitism. According to this interpretation, immunity is a hereditary defensive adaptation of the host to a *particular* species of parasite which formerly preyed upon the host's ancestors, but an adaptation in whose genesis the defensive reactions of those ancestors played no part. That interpretation is excluded by our analysis of parasitism in relation to anti-Lamarckian hypotheses respecting the origin of hereditary variations.

The second interpretation referred to above, however, is to the effect that races or individuals might have hereditary constitutions, not influenced by the functional or by the selective effects of the given infectious disease, which nevertheless were immune to that disease. This possibility is attested by the fact that many organisms producing diseases in members of one species apparently have never produced diseases in other species of organisms. The latter simply never supplied to the parasites in question conditions under which they could maintain and reproduce themselves. Moreover, some parasites of a given species do not produce diseases therein, but harmful effects of other sorts. They often of course subsist on various juices of the host with-

out liberating toxins in the blood or feeding upon the host's tissues.

We are not prepared to consider the question whether this interpretation really applies to any of the demonstrated immunities to infectious diseases, in races or individuals of the human or other species. We suspect that data are lacking for a decision between this and the Lamarckian interpretation of such cases. Experiments could no doubt be devised, and perhaps such experiments have actually been undertaken, to throw some light on the question. We might hazard the suggestion that experiments (working with pure-lines) designed to test the transmissibility of acquired immunity, both through the male and the female, might yield positive results, particularly if statistical methods were employed to test the possibility that such immunity might have a cumulative influence on the hereditary constitutions of successive generations acquiring immunity.

Until some such experiments are carried out (if they have not been already), we cannot say whether the immunities or partial immunities to particular diseases which have been inherited by particular races or individuals came as a result of defensive reactions to those diseases by the ancestors of the individuals or races in question, or whether they were produced by hereditary changes, in the genesis of which those diseases were in no wise involved.

These considerations do not of course invalidate our former analysis of parasitic adaptations, and of hereditary counter-adaptations thereto on the part of the host species, though they may be taken as qualifying, provisionally, the analysis of hereditary immunity considered as an example of such counter-adaptations on the part of the hosts. They cannot be taken as qualifying the analysis to which the germinal interpretation of parasitic adaptations was there subjected.

The fact that acquired immunity to disease is often transmitted to offspring through the mother but not through the father does not seem to have any particular value as evidence on the question here under consideration. In view of the stability of hereditary structure, we should not expect the immunization of one (or even of several) ancestors to have any appreciable effect on the corresponding *hereditary* characters of the offspring; but it is quite possible that such immunization, if repeated for a number of generations, whether in the fathers or mothers or both, might have such an effect. If we read Professor Guyer's experiments aright, the fact that immunity is not known to be transmitted through the male parent loses much of its interest for the question here at issue. Guyer produced defective lenses in rabbits' eyes by means of fowl-serum sensitized to the rabbit lens, and this defect was

found to be transmissible through the father as well as the mother, the defect behaving as a Mendelian recessive.[1] The significance of these results for the present question should be obvious.

In leaving this topic it should be pointed out that, as in the case of traumatic injuries, the evidence from pathology can in no wise invalidate our analysis of hereditary active adaptations to the environment, although it may aid us in defining the limitations on the influence of functional activity in the genesis of these characters. Insofar as hereditary immunities represent active adaptations to specific features of the environment, our general analysis will apply. Where immunity to a particular disease has not been acquired by species, races or family stocks, functional activity, in conjunction with other factors, has not succeeded in generating active adaptations of the sort which it has produced in other cases—assuming that hereditary immunities come as a result of defensive reactions to the several diseases in question. On the supposition that hereditary immunities may be so accounted for, the failure of a stock, race or species to establish such immunities for particular diseases might be due either to the comparatively brief periods of time during which it has been exposed to them, or to hereditary incapacity for the development of such immunity. No such considerations, as aforesaid, invalidate our analysis of hereditary active adaptations which have actually been produced.

Another species of evidence deemed adverse to the Lamarckian hypothesis is drawn from the negative results of grafts of various kinds. When the branch of a fruit tree is grafted upon a tree of a different variety, the fruit borne by the former is not affected by the stock upon which it is grafted. Similar results have been yielded by animal grafts. Harrison grafted together two parts of tadpoles from different species, and both parts retained their peculiarities until the adult stage was reached.[2] So far as we know, no effect of any such grafts on the hereditary constitution of offspring has been reported. More extensive experiments with the transplantation of ovaries from one female to another have been undertaken, and with apparently negative results. Guthrie experimented on hens [3] and Magnus on rabbits [4]

[1] Guyer, M. F., "Immune Sera and Certain Biological Problems," *Am. Nat.*, Vol. LV, 1921, pp. 97–115; Guyer, M. F. and Smith, E. A., "Transmission of Eye-Defects Induced in Rabbits by Means of Lens-Sensitized Fowl-Serum," *Proc. Nat. Acad. Sci.*, Vol. 6, No. 3, March, 1920, pp. 134–136.

[2] Harrison, R. G., "Experimentelle Untersuchungen, etc.," *Arch. f. Mik. Anat.*, Vol. 63, 1903. Cited by Conklin, E. G., *Heredity and Environment*, revised second edition, p. 350.

[3] "Further Results of Transplantation of Ovaries in Chickens," *Jour. Exp. Zool.*, Vol. 5, 1908, pp. 563–576.

[4] *Norsk Magazin for laegevidenskaben*, No. 9, 1907.

"obtained what seems to be a modification of the coloration of the offspring, due to influence exerted by the foster-mother upon the germ cells liberated within her body from the introduced ovary." But Davenport, Castle and others have shown that these experiments did not exclude the possibility that the offspring came from ova liberated by the regenerated ovaries of the foster-mothers themselves and not from the transplanted ovaries. Although this criticism was combated strenuously by Guthrie himself, the net result of the controversy has been to cast doubt on any positive results hitherto reported from transplantation experiments.[1]

On the other hand, Castle and Phillips secured negative results of an unequivocal nature, so far as they go, by transplanting the ovaries from a female guinea pig with black coat color (dominant character) to an albino female (recessive character) and afterwards mating the latter with an albino male. Pure races were used for this experiment, whose inheritance-behavior had been observed for a number of generations, thus excluding the possibility of ambiguous results from employing animals heterozygous for the characters in question. From this union two black pigmented young were produced, apparently similar in all respects to young from a black pigmented mother.[2]

It is not necessary for our purposes that we should enter into any extended discussion of these results. A few observations will suffice to define their bearing on our analysis. First of all, the transplantation experiments which have been carried through with the requisite care are not sufficiently extensive to permit of any very safe generalizations with regard to the subject. Such experiments would need to be extended to many other species of animals and plants, and continued for a number of generations before any really cogent evidence could be obtained from them. There might be a cumulative somatic influence of the foster-mothers (from the same strain) on the transplanted ovaries (from another strain), which would show up in some *n*th generation of offspring, were the experiments continued long enough.

So far as we are able to judge, *negative* results from such experiments, no matter how long the series of generations for which they

[1] See Castle, W. E. and Phillips, J. C., "A Successful Ovarian Transplantation in the Guinea-Pig, and Its Bearing on Problems of Genetics," *Science*, N. S., Vol. XXX, 1909, pp. 312–313; Guthrie, C. C., "Guinea Pig Graft-Hybrids," *ibid.*, Vol. XXX, pp. 724–725; "On Evidence of Soma Influence on Offspring from Engrafted Ovarian Tissue," *Science*, N. S., Vol. XXXIII, 1911, pp. 816–819; Castle, W. E., "On 'Soma Influence' in Ovarian Transplantation," *Science*, N. S., Vol. XXXIV, 1911, pp. 113–115; Davenport, C. B., "The Transplantation of Ovaries in Chickens," *Jour. Morph.*, Vol. XXII, 1911, pp. 111-112.

[2] See papers cited above; also, by the same authors, *On Germinal Transplantation in Vertebrates*, Carnegie Institution Publication No. 144, 1911.

were repeated, would not be a conclusive answer to the question whether the soma of the foster-mother might have an effect on the hereditary characters of offspring from transplanted ovaries. For there is, and in the nature of the case can be, no criterion for determining the number of generations for which the transplantation experiments would have to be repeated in order that the possible cumulative influences of the foster-mother on the offspring might show up in the latter. We simply do not know, for any species of organism, how resistant the genes or factors are to somatic influences. For all we know to the contrary, they might for various species, varieties or pure-lines be noticeably affected at widely different intervals of time, under the cumulative influence of the foster-mothers—say, for various species, etc., at the second, the tenth or even the thousandth generation. It seems clear, therefore, that such experiments can yield no very valuable evidence for problems pertaining to the origin of variations.

Even if one could be sure that the question had been settled for a given species, or for all species, the results, to our way of thinking, would still be of minor significance for the problem under consideration. If the results were negative, they would only prove that there are limitations to the influence of functional activity on the genesis of variations, but they would prove just as conclusively that there are limitations to the influence of physicochemical and morphological factors on the same process, for factors of these two classes are just as essential elements of Mendelian characters as is functional activity. Likewise, should any valid positive results be derived from such experiments, they would not, and for the same reasons, demonstrate a greater causal efficacy for functional activity than for physicochemical and morphological factors, in the genesis of the changes in the question.

Perhaps the most significant point which emerges from these considerations is that evidence of this sort has little or no bearing on problems pertaining to the nature and function of the primary factors in the genesis of variations. Functional, morphological and physicochemical factors cannot in such cases be distinguished for purposes of analysis, as they can be, for example, in the case of active adaptations. This is due to the fact that the latter category of hereditary characters have (as the former do not have) an explicit reference to specific features of the environment, whence it is possible, by an analysis such as we have set forth, to determine, at least in a general way, the rôles played in the genesis of variations by interactions of the organism with the environment, as well as by the environment itself. By supplementary analyses on the basis thus provided, the general rôle played by

organizatory factors as well as by purely physicochemical factors can also be determined.

Substantially the same considerations apply to the argument drawn from the clear segregation, in the germ cells, of factors for dominant and recessive characters, though these be combined, by crossing, generation after generation. In at least one group of experiments such crossing was effected for a number of generations. "Marshall and Muller kept flies heterozygous for three recessive mutant factor[s] for about seventy-five generations, and at the end of that time found that these factors had not been weakened in any way as a result of juxtaposition with their normal dominant allelomorphs." [1] This rule holds generally, so far as the evidence goes, for the apparent exception found in blends of various kinds has of course been explained consistently with the principle of segregation, by ascribing them to various combinations of two or more pairs of factors in those characters. So far as we can see, there are no grounds for the supposition that the recessive factors should be submerged rather than the dominant factors, assuming that either of them could be overpowered by the other; for both factors are potent, and perhaps equally potent, in the determination of the somatic characters, though the dominant factors appear to be more potent therein. Certainly both of them are essential, and it is hazardous and, we think, logically unsound to suppose that one of two essential factors in a process or phenomenon is *more* essential than the other.

But that is a minor consideration, so far as the present problem is concerned. As in the case of transplantation experiments, we do not know, and perhaps we never can know, whether dominant and recessive factors might not have an influence on one another, if combined for a *sufficient* number of generations. We have no way of determining the number of generations for which this combination would have to be made before such an influence manifested itself, granting its possibility. By more extensive experiments repeated, for different species, varieties of pure-lines, for a greater number of generations, we might arrive at some conclusions respecting the matter, of a greater or less degree of probability, but we can see no way of making such conclusions absolutely final, unless positive results should be secured.

And, as in the case of the transplantation experiments, decisive negative results, even if such could be attained, would have no particular significance for the problem with which we are concerned. For there is no way of distinguishing, for purposes of analysis, the func-

[1] Morgan, T. H., *The Physical Basis of Heredity*, p. 35.

tional, morphological and physicochemical factors in the genesis of Mendelian characters, and apportioning credit among them for the determination of hereditary modifications in those characters. The validity of this premise is not affected by the question whether factors for dominant and recessive characters could or could not affect one another by being combined together for some x number of generations necessary to test this question.

Semon has attempted to explain the negative results in all such cases by arguing that only *morphogenetic* stimuli are involved therein and that stimuli of this type are too weak to produce any appreciable hereditary modifications in the offspring.[1] To our way of thinking, no such characterization of the stimuli in the case can be made, as morphogenetic, physicochemical and functional processes are inextricably associated together therein. Just because of that, these results yield no clear evidence respecting the causal efficacy of the various factors involved in the origin of variations. They only show that certain characters of the organism are apparently possessed of an exceptional stability, a stability which is apparently refractory to influences of a certain kind, but whether absolutely so we have no means of knowing. These results do not show that such characters are equally resistant to influences of a different kind. The phenomena of mutations demonstrate, indeed, that these characters are not fixed, and that factors of one kind or another do induce modifications thereof. If our general analysis be sound, those factors would be partially classifiable under the category of functional activity as well as the coördinate categories of morphogenetic agencies and physicochemical processes. Nevertheless, it may be that functional activity does not have a representative influence on the genesis of these particular hereditary characters, although we do not have sufficient evidence to determine whether this be so or not.

In any case, no conclusions which may be drawn from these results can affect the validity of the analysis to which hereditary active adaptations have been subjected. That is of course the point with which we are mainly concerned. At the most, the negative results of transplantation experiments and of the combination, in a series of generations, of dominant and recessive factors could only indicate limitations to the operation of the Lamarckian factor in the genesis of variations. Such a service would not, we believe, be unacceptable to the advocates of a modified type of Lamarckism such as we are expounding here; but, for reasons already stated, it is doubtful whether even such a contribution to a theory of variations can be gleaned from these results.

[1] *Vererbung "erworbener Eigenschaften,"* p. 171.

A special argument against the Lamarckian hypothesis has been based on the fact that visual images of quite stable objects in the environment are not inherited. "Human life," says Thorndike, "offers a favored case for transmission of an acquired trait where transmission has clearly failed. The congenitally blind from eye defects do not have visual images of the sun, stars or any other of the permanent objects of the natural world, yet their ancestors for at least hundreds of generations, save in the cases of those lacking in visual images, had such images again and again. If the hourly experiences of hundreds of ancestral generations do not become a part of inborn equipment, we could hardly expect anything to do so." [1]

Before considering this objection, the fact cited may be assimilated to a large and important class of similar facts, facts summarized by the general law that the organism does not inherit sensory images of any kind, or indeed any knowledge derived from sensory experiences as such. Motor responses having reference to particular classes of objects or situations are often determined by heredity, though not sensory images of particular objects, nor composite pictures or images of classes of objects.

It seems to us that these facts not only do not disprove the transmissionist principle, but that on careful analysis they yield evidence favorable to that principle, besides indicating certain limitations on the scope of its operation.

For one thing, the inheritance of visual images in any considerable measure would probably be detrimental to the organism so endowed. If sensory images were handed on from parent to offspring, and other images acquired by the individual for himself, they could not be as coherent with each other and as reliable guides to action as when derived from the individual's own sensory experience alone. It might not harm much if he inherited visual images of the sun, moon and stars, but neither would it help much. And only a limited number of such images could be inherited without the individual being considerably inconvenienced by it, since if he could not discriminate between the trees, animals, etc., which he had seen, and those which his ancestors had seen, his action would necessarily be rather confused and halting, as a consequence.

The hypothesis whereby we would account for the facts in the case may appear a rather easy device for meeting the difficulties presented thereby to the Lamarckian theory, but it nevertheless seems to fit the facts better than does any alternative hypothesis. We believe the non-inheritance of visual images illustrates the regulative functions, in

[1] *The Original Nature of Man*, p. 234.

phylogenesis, of the morphogenetic or organizatory factors which are associated with physicochemical and functional processes, in all vital phenomena. This is a special application of a general hypothesis to be expounded more fully later, namely, that organizatory agents in vital phenomena can and do, within limits, build up structures and systems of functional activity requisite to the maintenance and perpetuation of life under the multifarious conditions to which it must adjust itself. In this process, such agents must work with physical and chemical conditions, both utilizing and building up protective mechanisms against those conditions, as the need may be, and guiding this process on the basis of their own experience or past interactions with these conditions. These agents must not only subject the given physical and chemical conditions to the specific organizations requisite to the fulfillment of the needs presented, but they must limit, where necessary, the influence which functional activity seeks to wield in its own right, if we may so speak.

This is not to deny that the organizatory agents themselves are not subject in their work to drastic limitations, which may often render the problems confronting them incapable of solution. The evidence goes to show that such limitations have been often imposed by material conditions, and by functional activity as well. Moreover, the organizatory capacities of those agents themselves may be so limited that they often cannot accomplish that which is demanded of them, under the given conditions. Limitations of two or all three types may be present. The extinction of many species of organisms when other species survived, the depopulation, with extensive geological changes, of vast areas of the earth's surface, the persistence of useless organs and of archaic instincts, the slowness with which adaptive characters necessitated by changed conditions of life are developed, when developed at all, go to show that all three categories of factors have imposed serious limitations on the synthetizing activity which is the peculiar office of organizatory agents in vital processes. But these agents have succeeded often enough in meeting their problems to have built up the world of life as it exists to-day, and as it has existed in the past.

May we not reasonably put down the exclusion of the inheritance of visual images as one of their successes? Granting provisionally the authenticity of our previous analysis, only one possible alternative seems to be left. It may be that, owing to material or other conditions of heredity with which we are not acquainted, visual images could not be inherited. If our analysis of active adaptations shall stand, and the operation of the Lamarckian principle be accepted as established for a wide range of vital phenomena, this is the choice open to us. It is per-

haps a matter of taste which of the two hypotheses shall be deemed the valid one.

The evidence adduced by Thorndike against the Lamarckian hypothesis takes on quite a different complexion when combined with other evidence necessary to bring out its true significance. This combined evidence tends to confirm the hypothesis just proposed, and to support a complementary hypothesis, in the Lamarckian sense, as to the rôle played in phylogenesis by sensory experience and motor reactions associated therewith. The other evidence referred to is taken from the phenomena of instincts, reflexes and other tendencies to action possessed by the organism prior to experience. Thorndike's own definition of such tendencies may be reproduced. "A typical reflex, or instinct, or capacity, as a whole, includes the ability to be *sensitive* to a certain *situation,* the ability to make a certain *response,* and the existence of a bond or connection whereby *that response* is made to *that situation.* For instance, the young chick is sensitive to the absence of other members of his species, is able to peep, and is so organized that the absence of other members of the species makes him peep." [1]

We cannot but believe that if Thorndike had attempted to appraise the significance of such phenomena for the problems of phylogenesis, he would have been led to suspect the validity of the inference he based on the isolated phenomena of the non-inheritance of visual images. For note that his definition of such tendencies affirms that the organisms possessing them are sensitive to certain situations prior to experience therewith. If our analysis of hereditary active adaptations, and especially of the young chick's hereditarily determined perceptions of objects in space, be valid, then sensory experiences together with the associated motor reactions have played a representative rôle in the genesis of reflexes, instincts and other tendencies, in which perceptions and the associated reactions are determined by hereditary factors. It is impossible to avoid the inference that in all such cases there is some sort of *recognition* of the objects or situations to which the organism is sensitive, and responsive, by heredity; and any imputed recognition of an object or situation without a sensory experience thereof in the past (on the part of the organism or of its ancestors) is a self-contradictory conception. It is likely, we think, that in all hereditary active adaptations, properly so-called, there is some sort of sensitivity to and recognition of the objects which the adaptation represents. Representation

[1] *The Original Nature of Man,* p. 6. (Italics ours.) Consider also in this connection the same author's study of the behavior of young chicks, and especially the different reactions of chicks four days old when placed, on boxes, at various distances from the ground: "The Instinctive Reactions of Young Chicks," *Psychological Review,* Vol. VI, 1899. See *supra,* p. 69.

and recognition would seem to be, for such cases, virtually synonymous terms.

We thus have the positive complement of the analysis supplied by Thorndike. The sensitivity, recognition, attention and response involved in any specific reflex, instinct or other original tendency having reference to a particular class of objects or situations are, on our analysis, the most cogent sort of evidence for the validity of the Lamarckian principle. On the other hand, the non-inheritance of visual images indicates limitations on the operation of that principle—limitations imposed by the physicochemical conditions or the organizatory agents involved in the exclusion of such inheritance, or by factors falling within both these categories. Perhaps the feature of greatest interest in this case is the indication it supplies as to the way organizatory agents function in phylogeny, especially the nature of the limitations imposed upon functional activity in its phylogenetic rôle, and, more generally, the disabilities, interferences and defects from which factors of any particular category may suffer, in the genetic processes in which they have a part.

We may say, finally, that the argument from the non-inheritance of visual images, even were it valid, would not affect our analysis of active adaptations, since the *inheritance* of such images would not constitute an adaptation of this type. On the contrary, the consideration of it has served to throw that analysis in sharper relief than before. For its examination has brought out the fact that other elements of sensory experience *are* involved in many if not most active adaptations and that those elements of experience have therefore played just the rôle that would, on the Lamarckian hypothesis, be expected of them.

Another special argument against the Lamarckian principle has been drawn from the fact that knowledge acquired by parents is never transmitted to their offspring, or at least not in a form or in a degree permitting its identification as such. This fact, like the non-inheritance of visual images, is quite compatible with the Lamarckian principle; and it supplies indications respecting the manner in which that principle operates in one important group of vital processes, as well as limitations on the operation of the principle in that domain.

As a starting point for our examination of this argument, reference may be made to Roux's contention that it would be a disadvantage to man were he to inherit knowledge from his ancestors, as his freedom of development (*Ausbildung*) would thereby be limited. Our principal superiority over other animals consists in our *Universitalität,* by which Roux means, we take it, the plasticity of our nervous organization.[1]

[1] *Der Kampf der Teile,* p. 37.

If this be true, we might perhaps be justified in saying that the organizatory agents operative in human evolution may thus have limited the rôle of functional activity in the genesis of our nervous and mental constitution. Functional activity will nevertheless be seen to have played in this domain a representative rôle of very farreaching importance, if the non-inheritance of individually acquired knowledge is considered in connection with mental functions which *are* hereditary, together with the multifarious features of the environment to which they have reference.

The individual does have to learn many things which generations before him had all thoroughly learned, which learning, it would seem, might have been transmitted. Indeed, individual learning is so conspicuous a determinant of behavior that many writers have been led to minimize and even deny (in certain directions at least) the operation of hereditary factors in behavior. A simple logical consideration, however, will serve to set the matter in quite a different light. The individual is certainly endowed at birth with mental traits, capacities, dispositions, etc., which *make possible* every type of behavior, and every species of learning, which he may later acquire. In other words, the individual is born with capacities for all the responses to the environment which are ever possible to him, irrespective of the rôle which his own experience plays in the determination of those responses. The point is emphasized by the fact that human responses and experiences are markedly different from those of other animals, and because of differences in their hereditary traits and capacities.

Moreover, all types of human behavior have reference to the environment, and, what is of greater significance in the present connection, they have, as one set of their determining factors, *more or less* specialized hereditary adaptations to the environment. The specificity of these adaptations varies, it is true, from congenitally definite responses—as in the reflexes—to highly generalized and plastic responses, as in the adaptations to the language and customs of the social group. All such adaptations represent, in some degree, distinguishable features of the environment. Indeed they will be deprived of all meaning whatever if one considers them apart from those features of the environment.

Learning plays infinitely diverse rôles in the determination of behavior of different types. Certain reflexes are but slightly modifiable by experience, while complex speech reactions are not possible without it. There are all kinds of gradations between these extremes. To all the more definite hereditary responses our previous analysis of active adaptations will apply. It applies also, we believe, to the more plastic

generalized adaptations, or those that must be transmuted into specific and useable adaptations through learning or experience.

While an application of our analysis to the latter class of adaptations will not yield such decisive results, perhaps, as has its application to the more specific hereditary adaptations, there are strong grounds for the belief that it applies to them as well. Insofar as the generalized plastic adaptations are correlated with more or less specific features of the environment, the former analysis obviously applies. That there is some such correlation is equally obvious. The real question, however, is why we have the plastic generalized adaptations instead of more specific adaptations. Stated differently, has not the Lamarckian factor failed to do what might have been expected of it, when it produced the more generalized instead of the more specific adaptations?

The reply to this question is to be found, we think, in a consideration of the features of the environment to which the more plastic adaptations have reference. We may lay it down as a general principle that, human beings (and other animals) are, by heredity, sensitive and responsive to the objects or situations, or to the *elements* of objects or situations, which have been stable or identical for long enough in the experience of the given evolutionary series for the adaptations to those objects or situations (provided these also remained the same) to pass over into hereditary structure. This general principle would doubtless require many qualifications in detail, to allow for the obstacles and interferences imposed by other factors, as in the case of visual images, for example. But a systematic survey of stable environmental objects and of hereditary responses, with a view to determining their correlations, would, we believe, substantiate the principle proposed. It would take us too far afield to attempt a survey of this nature, and it could not, owing to unsettled controversies over the nature of instinct, be completed at the present time. It is, however, a problem worthy of investigation. We shall perforce be content with the observation that, subject to the general qualification stated, all stable objects or situations in human experience, which for a prolonged period of time provoked fairly uniform responses thereto, appear to have correlates in fairly definite congenital responses of the human organism. Those who may wish to go into the matter systematically should be cautioned against a possible source of error. It should not be assumed that objects instinctively attended and reacted to must be as *specifically* distinct as those which sensory discrimination and scientific analysis are able to define. They are sometimes specifically distinct, but more often they will be found to belong to *generic* classes, classes formed on the basis

of likeness of response, rather than on the basis of qualities distinguishable by the senses and the intellect.

The plastic generalized adaptations, on the other hand, will have reference to objects or situations whose forms or elements have not been stable, but which have taken on new forms or included new elements which rendered them relatively novel to what may be called hereditary recognition and response. This instability is very largely, though not entirely, due to a group of capacities and tendencies which enable and impel men to *alter* their environment. How great a rôle these capacities and tendencies play may be realized by considering the revolutionary changes in environment which take place in the same locality in a single generation. Indeed, the alterable phases of the environment are almost never the same for many generations together. And most superficial features of the environment are alterable. Man even creates for himself special environments in such fundamental matters as temperature, humidity, foodstuffs, flora and fauna. The same thing is notoriously true of language, customs, traditions, institutions, arts, etc., which so largely constitute the social environment. Change is of course gradual in this domain, but it is cumulative and profound, if taken for the period of time which we may believe necessary to the establishment of modifications in hereditary mental traits. Compare, for example, the English language in Chaucer's time and our own; industrial arts then and now; manners, fashions and ideas in his day and ours. Yet, had all these features of the environment remained the same from Chaucer's day to ours, they could scarcely have induced identifiable modifications of hereditary mental characters in individuals now living, all of whose ancestors for the period in question had reacted to those features of the environment.

The hereditary factor corresponding to these man-made changes in the physical and social environment is precisely that plasticity of nervous and mental organization which is invoked against the hypothesis that experience has anything to do with the genesis of mental traits and capacities. That plasticity is just what we should expect on the Lamarckian principle. There was, of course, plasticity to begin with, or at least a capacity for the development of placticity—a factor which must of course be subsumed under our general category of organizatory agents; but this plasticity has apparently been developed and more or less specialized with reference to the environment, through its interactions with the environment. The world which our ancestors learned and lived in is precisely the sort of world which we are adapted by our mental traits and capacities to learn and live in. Moreover, there are

strong grounds for the belief that we are better adapted by heredity for life in the sort of world our *remote* ancestors lived in—before change in the physical and social environment had fairly set in—than in the complex world of the present day, which is the product of many generations of social life. If that is the fact, it may be taken as illustrating the operation of the Lamarckian principle in our racial past, and also the limitations on the operation of that principle. Apparently it has not been able to rid the race of archaic instincts, or to strengthen in any marked degree instinctive tendencies better suited to our modern social environment, or to develop the higher order of intellectual capacities so urgently needed for the solution of modern problems.

There is a very close parallel between hereditary sensory functions, and the plastic cognitive functions involved in learning. We do not inherit actual perceptions and visual images, but our sense organs are receptive to certain sorts of stimuli presented by the environment. The retina is sensitive to light waves whose rates of propagation fall within certain limits, the auditory apparatus to air waves with amplitudes coming between a certain minimum and maximum, "taste buds" to a limited number of chemical substances, the temperature sense organs to temperatures within a certain range, and so on. From the sensory qualities corresponding to all these stimuli are built up, with the aid of other mental functions, the physical world as we know it. Experience is of course absolutely essential to this process. Both together give us the physical objects of our environment. If our former analysis be correct, sensitivity to certain specific stimuli presented by the environment cannot be adequately interpreted phylogenetically without assigning to functional activity a causal rôle in the process. Yet perceptions of trees, houses, tools and other objects are not inherited. The explanation seems to be, as pointed out in the discussion of visual images, that such inheritance would have been disadvantageous, and the organizatory agents involved were successful in averting it. Nevertheless, as in the case of the young chick, something more than sensitivity to stimuli of various kinds is inherited. Something of the meanings attached to particular classes of sensations—their significance for action—seems to have been inherited. That appears to be true of all hereditary responses to specific objects or situations.

It is legitimate to interpret other learning processes in a similar fashion. The principal difference between the hereditary adaptations at the bases, respectively, of perception, and of learning concerned primarily with *relationships* between perceptions, is that the latter are much less specific than the former. Perceptions are the synthesis or

organization of very *specific* sensations, while learning the relationships between perceptions or objects involves the progressive specialization of *generalized* cognitive functions, and the synthesis of functions thus specialized. There is of course no separation of the two types of processes, in the development of the individual, but there is a significant distinction between the hereditary factors involved.

We may say that in both cases the hereditary traits and capacities involved represent a *high degree of adaptation* to the environment. Hereditary knowledge of relationships which are rapidly changing—as in the social environment and many features of the physical environment—would have been both impossible and injurious. The inheritance of instinctive responses to stable objects of the environment, and of developed perceptive faculties, as in the case of the young chick, represents about all that was possible or useful in this direction. On the other hand, the genesis and inheritance of very specific sensitivities to light waves, air waves, etc., were possible and useful because of the stability of these stimuli. Likewise the genesis and inheritance of plastic cognitive functions were possible and useful because of changing phases of the environment, while the genesis of more specific adaptations to these phases of the environment was not possible because of their instability, and would not have been useful, had it been possible.

The limited potency of functional activity in modifying hereditary structures and the regulative functions of the organizatory agents in phylogeny seem to have been jointly responsible for the results in both cases. It is possible and indeed probable that the physicochemical factors involved in this process played a rôle of coördinate importance. Perhaps we may ascribe to factors of the latter category, at least in part, the persistence of tendencies which are largely archaic, and the slowness with which more useful traits and capacities are strengthened or developed, or failure to develop such characters at all. The weight to be given factors of these several categories, in a genetic interpretation of the processes in question, is a matter which could not now be settled, of course, owing to the meagreness of the evidence bearing upon it.

These practical limitations of our analysis, though developed in connection with the special problem we have been considering, apply also to problems pertaining to the genesis of other active adaptations and, in a general way, but with some possible exceptions, to the genetic interpretation of other types of variations.

Before leaving the special topic under consideration, reference may be made to the orthodox Lamarckian position respecting the noninheritance of language and other sorts of knowledge. This is to the

effect that such knowledge is not inherited because of its complexity. The explanation in this form is little more than a restatement of the fact to be explained, as will appear if it be reduced to the equivalent statement that complex knowledge is not inherited. Such a statement, besides explaining nothing, actually fails to allow for many of the facts in the case. Many insects are endowed with very complex instincts, and such instincts carry with them a species of knowledge, while our own hereditary mental functions are quite complex, though they do not approach in complexity the responses developed through experience. It may be that there is a limit to the number and complexity of the mental functions that can be inherited, but, even so, that would not serve to account for the actual inheritance of many mental functions which *are* complex, and the non-inheritance of many others. Answers to such questions must be sought, as we have attempted to show, in a consideration of the conditions under which various mental traits and capacities have functioned in the past, and of the rôles which the various factors in phylogeny have probably played in the genesis of these characters.

Perhaps our analysis of the plastic, generalized adaptations underlying our learning processes and the acquired responses correlated therewith may be taken as applying, with necessary qualifications, to other adaptations of the same sort, and as meeting objections to the type of analysis here set forth, which might be based thereon. All these adaptations fall within the scope of our analysis, however wide or narrow the range of the environmental stimuli to which they have reference. And wherever the adaptations are not as specific as might be expected on the Lamarckian principle, the explanation would probably be found in interferences, retardations or limitations imposed by physicochemical and organizatory agencies involved in the genesis of those adaptations.

We have now examined the more important arguments against the Lamarckian hypothesis, as drawn from the non-occurrence of hereditary modifications which might have been expected according to that hypothesis, and found that, in the present state of our knowledge, none of them are destructive of the hypothesis in the modified form advocated here, and, more particularly, that they do not bear against our analysis of active adaptations and its support of the Lamarckian principle. But our consideration of those arguments has furnished many indications of specific limitations on the operation of that principle, and of the specific rôles played in the genesis of variations by other factors.

We now turn to a consideration of objections drawn from active adaptations in the genesis of which functional activity has apparently played no rôle, or at least not one of a representative character.

(2) Active Adaptations Apparently Inexplicable on the Lamarckian Hypothesis

The elaborate arrangements of flowering plants which assure cross-pollination through the visits of insects are a group of specific hereditary adaptations in whose genesis functional activity apparently could not have played a representative rôle. This group of characters is very widely distributed in the plant kingdom, and the structural arrangements in question are often quite specific, being adapted in some cases to single species of insects. It is clear, therefore, that any theory respecting the origin of variations which shall have general validity must account for these facts. A fact of crucial significance for our theory is the passive rôle of the flowers themselves in respect to the transfer of pollen from one plant or flower to the stigma of other plants or flowers. From that point on to the actual union of male and female germ cells floral organs play a more active rôle, but the more striking floral adaptations are those which enlist the coöperation of external agencies in effecting cross-pollination, without the flowers themselves playing an active rôle therein.

These adaptations are of course quite different from the types of adaptations considered hitherto, for the latter are all *active* adaptations, connoting structures responsive to more or less specific environmental stimuli. We argued that those adaptations must *represent* the objects of the environment with which they are correlated; and that any hypothesis accounting for the genesis of those adaptations without assigning to prior interaction of the ancestral structures with the environment a representative rôle in the process leads to insupportable consequences, whereas the Lamarckian hypothesis in the form presented does not involve any such consequences.

Now, many floral adaptations imply quite similar representations of specific environmental features, but they exhibit no *active* responses to these features of the environment, and so prior reaction of the ancestral tissues or organs which evolved into the present structures seems to have been excluded from the genetic process altogether. Our theory apparently fails therefore to account for this very important group of characters.

Let us take a case where the floral adaptations are of the most specific sort, that is, where their efficacy is dependent on the behavior of a single insect species. In some orchids the floral arrangements are such that cross-pollination can be affected only by particular species, "the adjustment between the two being carried remarkably into details." Similarly, the cross-pollination of *Yucca Whipplei,* and indeed fertiliza-

tion of its ovules at all, is dependent on a definite sequence of instinctive acts by a certain moth of the genus *Pronuba,* which behavior series is itself an indispensable part of the moth's own reproductive activities. So close is the interdependence of plant and insect that the latter is usually designated the Yucca moth. The reproduction and hence the very existence of the two groups of organisms are indissolubly bound up together, so far as we can see. We may take this as an example of specific floral adaptations presenting difficulties to our theory at least as great as any other case that could be cited.[1]

The adaptations in this case are quite analogous to the reciprocal adaptations between many parasites and their hosts, and an identical analysis could be applied to it. We saw that no hypothesis rejecting the Lamarckian principle could supply a tenable interpretation of parasitism, and the same conclusion would seem to hold for the present case. Yet functional activity on the part of *Yucca Whipplei* was apparently excluded from the genesis of its adaptations to the Yucca moth. We thus have an antinomy which is apparently insoluble.

The way out will perhaps be indicated by an examination of the apparently irreconcilable propositions of the antinomy in order to determine whether one or both of them may be so modified as to remove the contradiction between them. A review of the analysis to which we subjected anti-Lamarckian interpretations of complex hereditary adaptations—and especially complex reciprocal adaptations between two distinct species—convinces us that a place must be found for the Lamarckian principle in the genetic process under consideration; for the evidence yielded by that analysis seems to us of a more cogent sort than evidence deduced from the passive rôle of floral adaptations in cross-pollination; and, as we shall see presently, the passivity of those adaptations is not incompatible with the hypothesis that functional activity has played a representative causal rôle in their genesis.

The problem may be solved by ascribing to the organizatory agents in the process the logically necessary functions therein which cannot be attributed to functional activity or to physicochemical factors. The inference is that those agents guided the morphological changes issuing in the given floral adaptations, and that those agents were themselves guided by the results of the interactions beween the tissues concerned, on the one hand, and the environmental agencies involved in cross-pollination, on the other hand. Those who have accepted the mechanistic hypotheses dominant for so long in biology will doubtless protest against such a proposition. When the implications of this hy-

[1] A brief summary of the facts in the case is given in Lloyd Morgan's *Animal Behaviour,* pp. 82–84.

pothesis are compared with the implications of the several alternative hypotheses, however, it will be found less unacceptable, we believe, than any alternative hypothesis.

A consideration of the many analogies to the inferred processes in this case, which may be drawn from men's activities in organizing their environments and, to some extent, their own bodies should make the acceptance of this account less distasteful to us. At any rate such a consideration will show that we are only applying in a domain apparently not far removed from our own conscious behavior, principles which are clearly operative in the latter. In alluding to these analogies we have assumed that human behavior cannot be interpreted in physicochemical terms alone, and that the organizatory factors in conscious behavior are differentiated from the organizatory factors in the genesis of variations. These assumptions will be duly considered in a later part of the inquiry. Our consideration of floral adaptations to specific environmental agencies, particularly our appeal to analogies drawn from human behavior, will have raised the question whether organizatory agents involved in the genesis of such adaptations and perhaps in vital processes generally are endowed with intelligence. Behavior guided by past action seems to imply some such conception. This problem will also be considered at a later stage of the inquiry.

More generalized adaptations of flowering plants to secure cross-pollination through the agency of insects, such as size, shape, color and odor of the flowers, the production of nectar, the presence of stamens and pistils on the same plant, the adhesive qualities of the pollen, and, less often, dimorphism, the sterility of flowers to their own pollen, mechanical arrangements presenting close-pollination, the ripening of stamens and pistils at different times, etc., might all be interpreted on the same principles. It would be tedious to apply our theory, with the necessary qualifications, to all these cases, and as its general applicability thereto is indicated by our analysis of the floral adaptations in the Yucca plant, we may dispense with any more extended consideration of these cases.

Extensions of the lung sacs, in most birds, into the bones are another group of adaptations, in the genesis of which functional activity apparently could not have played a rôle of a representative character. The wide distribution and obvious utility of this character make the problem of its genesis one of great theoretic importance. As the interpretation of the case on our theory seems to be quite similar to that just set forth for floral adaptations, the application of the theory thereto may be indicated in a somewhat summary fashion. Obviously an explanation of this character on the hypothesis of "use and disuse,"

as usually conceived, could not be very plausible, considering the hardness of the bony structures modified, the negligible amount of mechanical pressure applied to these structures through the functional activity involved, and the absence of such characters in allied classes of animals with a different mode of locomotion.

As these adaptations are of a generalized nature, it might be supposed that chance germinal variations having no particular reference to the environment could have resulted in the establishment of these characters. In view of the probable common origin of the groups falling within this class, and the very real advantage which the character in question conferred on its possessors, it is unnecessary to suppose that any very large number of chance variations, or series of variations, must have occurred independently of one another, in different organisms, to account for the wide distribution of this character.

Certain facts, however, indicate that this is not the correct explanation. For one thing, this character is not found in other groups of lung-breathing vertebrates. So far as we can see, there is no reason why it should not have appeared in those classes of vertebrates, were chance variations the originating cause. After the character had developed to a useful stage in the birds, selection might have preserved possible chance variations in the direction of still greater utility. But a considerable development of pneumatic cavities in the bones would have been necessary before these carried any appreciable survival value to their possessors. Within the limits of that development, so far as we can see, there might have been an equal amount of chance variation in other groups of lung-breathing vertebrates, in the direction of the given character. The positive disutility to allied groups of vertebrates, within the limits of the development not appreciably useful to birds, would have been negligible, if we can suppose that there would have been any disutility at all. If this analysis be correct, we must look elsewhere for the causal factors in the case.

The more significant of these causal factors would seem to come under the categories of organizatory agents and functional activity. We can see no escape from the conclusion that organizatory agents directed phylogenetic processes in this class of organisms toward the character under consideration. And, as in floral adaptations, the organizatory agents operative in the case must have been guided by the results of prior interactions between the agents and tissues involved, on the one hand, and the environmental factors represented in the adaptation, on the other hand. An application of our former analysis of intra-organic adaptations to this case will serve to emphasize the rôle which functional activity has played in the process. The coadaptations

of the structures involved in this character could not be explained, according to that analysis, without assuming that the coöperative functioning of the phylogenetic predecessors of those structures played a causal rôle in the genesis of the latter.

One might continue the citation of somewhat similar cases, without ever coming to the end of them. The list would include the poison-glands of snakes; the tusks of *Babirussa alfurus;* the tongue of *Chamælon vulgaris,* so useful in the food-getting activities of that animal; the peculiar position of the anus in *Fierasfer,* a fish living in sea cucumbers, and its mode of voiding its fæces; hair on the tip of the tail and in the ears of many mammals; the light-organs of many animals living in abyssal depths of the sea; the loss of hair and acquisition of a thick outer layer of fat by the whale; the adaptedness of mammals' teeth to their food habits, and so on.[1] Limitations of space preclude our consideration of arguments drawn from these and other similar cases, against the Lamarckian hypothesis. The line which such a consideration would take has been sufficiently indicated, perhaps, by our analysis of characters falling within the same general category, and presenting comparable difficulties to our general theory.

To avoid misunderstanding, it should be emphasized once more that in our consideration of such cases we are not claiming to supply anything like a complete account thereof. The aim of our inquiry is to expose hypotheses respecting the origin of variations which we regard as fallacious, owing to the unsound assumptions on which they rest; and to formulate, in general terms, assumptions and hypotheses which shall not suffer from the same defects. This we believe to be an indispensable prerequisite to the most fruitful scientific work in this field. But we are not purporting to offer more than premises and general hypotheses upon which exact scientific work may in the future be based. Doubtless many of the special corollaries which we have ourselves deduced from the general premises and hypotheses laid down, by way of illustrating and justifying these propositions, will be disproved or modified by future investigations. In the consideration of cases presenting serious difficulties to our general theory, we have attempted to show that these difficulties are more apparent than real, and that all such cases can be fitted into our theory without undermining it. Our examination of those cases has also rendered the positive service of indicating the specific and variable rôles apparently played in phylogeny by factors coming within the same general categories.

We may now indicate the position of our general theory with regard

[1] For these and many other cases, see Spengel, J. W., *Zweckmässigkeit und Anpassung,* pp. 13–15, and Plate, L., *Selectionsprinzip,* 1908 ed., pp. 454 ff.

to the restitution of mutilated parts and the phenomena of regeneration in general. Morgan has shown that, while the capacity for regenerating lost parts may be useful to an animal, such a capacity could scarcely be accounted for on this basis. For in some cases regeneration is so slow that it confers little or no advantage in the struggle for existence, while in other cases regeneration occurs in such a way as to be a positive handicap. Also regeneration takes place in a great number of regions and, in various cases, from embryonic stages of development onwards. Parts regenerate that could scarcely have been liable to injury under normal conditions, indicating, again, that "natural selection" can not be credited with having originated this character.[1] These facts also rule out the Lamarckian factor, in its orthodox sense, as an originating cause of this character.

A survey of the phenomena of regeneration points to the conclusion that though external factors often partially determine processes of regeneration,[2] the capacity for regeneration, whether extensive or not, is to be ascribed in the main to the morphogenetic properties of the species possessing it.[3] Since, however, we do not have regeneration in general, but widely distributed capacities, in various species of organisms, for the regeneration of specific structures, all of which are the product of evolutionary processes, the general property of regeneration must be related to the primary factors in organic evolution.

The relationship, in brief, seems to be this: Regeneration may be viewed as a species of reproduction, a property which is not wholly lost by the somatic tissues of even the more complex organisms, with the progressive specialization of these tissues on functions other than reproduction. This property is carried along with a good many of these tissues, if we may so speak, and replaces them if they should be lost. In more acceptable terms we may say that many groups of organisms have the capacity of regenerating a smaller or greater number of specialized parts, a general capacity which is retained even though these parts be profoundly modified and the capacity for regenerating particular parts be lost. To speak again in figurative terms, that capacity accepts phylogenetic changes as it finds them, and proceeds to adjust itself to them as best it may.

The capacity for regeneration is thus a fundamental property of many organisms, in the origin of which functional activity, another fundamental property, played no part, although functional activity is

[1] Morgan, T. H., *Regeneration*, pp. 107 ff.

[2] Morgan, T. H., *op. cit.*, Chap. II.

[3] *Ibid.*, pp. 258–259. Regenerative processes cannot be interpreted in terms of known physical and chemical principles, as Morgan rather reluctantly concedes.

involved in specific regenerative processes. But the general property becomes specialized with reference to specific structures and functions in whose genesis functional activity, according to our analysis, has played a part, and one of a representative character. The newt did not possess the capacity for regenerating its leg, until the newt itself, including its leg and the many specific adaptations which the leg represents, was evolved. We seem justified in saying, on the basis of this very general analysis, that nothing in the phenomena of regeneration is incompatible with the theory of phylogenesis herein set forth.

Certain of the tropisms constitute another group of characters in whose genesis functional activity seems to have had no part. Galvanotropism is perhaps the most striking example of this group, and apparently presents quite serious difficulties to our theory. "If a galvanic current is passed through a trough filled with water, and animals are placed in this trough, it can be observed that an orientation in relation to the direction of the current takes place in many of the animals, and that they move in the direction either of the positive or of the negative current . . . Galvanotropism is, however, purely a laboratory product. With the exception of a few individuals, which have in recent years fallen into the hands of the physiologists who happened to be working on galvanotropism, no animal has ever had the chance to come under the influence of an electric current. And yet galvanotropism is a remarkably common reaction among animals. A more direct contradiction of the view that the reactions of animals are determined by their needs or by natural selection could hardly be found." [1]

We have in these phenomena *active* responses by the organism to an environmental stimulus, and responses set in motion by a stimulus to which the ancestors of the reacting organism were never subjected. It seems, therefore, that our general theory must break down when confronted by this fact, for it holds that all hereditary active adaptations to specific features of the environment were generated by and through the reactions of the ancestral organisms to those features of the environment.

A clue to the solution of the problem is supplied by a consideration of phenomena which Loeb ignores or takes for granted, without canvassing their significance for a systematic genetic interpretation of the responses in question. First of all, organisms react in one way or another to a wide range of stimuli, when first subjected thereto, which none of their ancestors ever encountered. The diversity of such stimuli and the varying reactions evoked thereby may be indicated by

[1] Loeb, J., *The Mechanistic Conception of Life*, pp. 50–51; University of Chicago Press, publishers.

a number of examples. The ingestion of chemical compounds not found in nature, the subjection of animals in our laboratories to chemical, thermal and other stimuli of sorts before unexperienced by any of their several species, the soldier on the modern battlefield, the savage set down for the first time in a metropolis like New York or London, the attacks of a new parasite quite unlike any which the host species has before encountered, will serve to illustrate the variety of new stimuli to which organisms may react, and the diversity of the responses to such stimuli. These reactions may be very specific, as we must suppose the behavior of the organism toward a new parasite or a new poison to be, or vague and random, as are the reactions of a savage to the stimuli presented by a modern metropolis.

In the next place, the reactions of an organism to any such stimulus are largely determined, and in more or less specific directions, by its hereditary characters. Of coördinate importance with hereditary functional and morphological characters are the specific chemical substances of which an organism and its tissues are composed, as well as the specific forms of energy prepared and utilized by the organism. These are all hereditary characters, and all are determinants of the organism's behavior when either wholly new or quite familiar stimuli are presented to it. Usually, the rôles which hereditary characters of these several classes play in determining the given response will not be equally conspicuous. In digestive processes, purely chemical processes are specially conspicuous; in habit formation, functional activity is the conspicuous factor; in regeneration, morphogenetic agencies are the more conspicuous. But it is certain that all three categories of factors are present in these and all other vital processes.

In galvanotropism, physicochemical factors are especially conspicuous, whether this phenomenon be considered from the side of the stimulus or of the organism. The behavior of animals orienting themselves in relation to an electric current is illuminated by a consideration of the probable chemical, physical and electrical changes induced in the organism by that stimulus. Insofar as this behavior can be accounted for in physicochemical terms, we should say that it represents *non-vital* or *pre-vital* reactions, responses or adaptations. True to his hypothesis, this is just the explanation that Loeb gives [1] of these phenomena. What happens *in* the animals subjected to an electric current could happen, in part, to the substances of which these animals are composed, were these *outside* animal organisms altogether. A certain complex of physicochemical substances simply reacts in certain specific ways to

[1] *Op. cit.*, p. 50; see also the same author's *Forced Movements, Tropisms, and Animal Conduct,* Chap. IV.

the stimulus in question, whenever those substances and that stimulus come in contact. Although Loeb is correct in saying, in effect, that the ancestors of these reacting organisms were never themselves subjected to an electric current, the problem would probably take on a different aspect were consideration given to the genetic relationships between electricity and *substances* of the sort entering into the composition of such animals.

However that may be, it is beyond doubt that reactions properly termed non-vital play a large part in this tropism. And so far as we can see, the possibility of those reactions, or the adaptation of the physicochemical substances of the animal to the electric current, was not established in connection with vital processes at all. These phenomena are of an order with the organism's response to an electric current so powerful, or to temperature so high or low, or to pressure so great that death takes the place of life. They fall into a broader category with osmosis, dialysis, filtration, surface tension, gravitational pressure, chemical disintegration of food substances and all the many types of physical and chemical reactions that occur within living bodies.

But there is another side of the story, and one which Loeb virtually ignores. He has centered his attention on the physicochemical processes involved, while taking for granted the *structure* of the organisms responding to the electric current, although showing how the structure, *as given,* partially determines the specific character of the responses in question. Now, if our general analysis be a valid one, both functional activity and organizatory agencies have played a rôle in the genesis of structure coördinate in importance with that of physicochemical factors themselves. If this be true, the specific behaviors of animals subjected to an electric current—such as characteristic swimming movements, for example—are determined as much by functional and organizatory as by purely physicochemical factors. A more systematic investigation of the question whether structure and functional activity themselves may not be accounted for in physicochemical terms will be undertaken in later chapters, and our assumptions respecting the nature of those factors must at this stage of the inquiry, therefore, be regarded as somewhat provisional.

A similar interpretation could be made of positive heliotropism in animals which do not live in light, and of other so-called tropisms in which the inducing stimulus and significant features of the response can be expressed in physicochemical terms. Such an interpretation would hold regardless of whether the given stimulus was entirely new or quite familiar in the racial histories of the organisms in question.

The tropisms do not appear, therefore, to contravene our general

theory, when all the factors therein are duly considered, but, on the contrary, to indicate, as have other cases deemed to bear against the theory, specific rôles played by physicochemical, functional and organizatory agencies in the genesis of an important group of characters. The tropisms and their significance for a theory of phylogenesis will be further considered in our examination of physicochemical conceptions of life.

Our previous remarks on passive adaptations and non-adaptive characters may be elaborated at this point.[1] Although we have largely restricted our discussion thus far to active adaptations, regarded as tests of current theories respecting the origin of variations, much of our analysis is applicable to characters not falling within this category.

We may observe, to begin with, that insofar as passive adaptations have genuine survival value, functional activity is involved in their perpetuation, since spines, stings, poisons, mimicry, protective coloration, hard body coverings, etc., derive whatever utility they possess either from their influence on the behavior of other species which might prey upon the organisms endowed with such characters, or from protection which they afford against possible injurious effects of inorganic factors in the environment. While the genesis of such characters cannot be directly interpreted on the Lamarckian principle, functional activity of a representative character is involved in their maintenance and even in their development, if we may regard the reactions of environmental agencies *to* the organism as in the nature of functional activity within the larger system constituted by the organism and its environment; for it is through such reactions that passive adaptations, and useful variations thereof, are passed on to succeeding generations. But the *genesis* of the discrete variations issuing in the passive adaptation would not, on our showing thus far, be explained on the Lamarckian principle.

Nevertheless passive adaptations are the products of active body tissues,[2] and these are all actively adapted in a number of specific ways to other tissues of the body. The activity of such tissues is inseparable from those specific hereditary adaptations, and the genesis of the latter cannot be adequately accounted for, as we have shown, without attributing to functional activity of a representative character a causal rôle in the process. Indirectly, therefore, functional activity of a representative character has been a factor in the genesis of passive adaptations.

[1] See pp. 111–112, *supra.*

[2] Haacke, W., *Grundriss der Entwicklungsmechanik,* pp. 289 ff. Cited by Kellogg, V. L., *Darwinism To-day,* pp. 270–271.

One could interpret in a similar fashion the necessarily almost perfect adaptations between the reproductive organs and germ cells of the two sexes in any sexually reproducing species. Each sex serves as the selective agency for variations of those fundamental characters in the opposite sex. Functional activity is obviously involved in this process, and functional activity of a representative character. Where a male or female of a sexually reproducing species has peculiarities in its germ cells or reproductive organs which disqualify it for reproduction, those peculiarities cannot of course be transmitted to offspring; but where a mate or mates are found with corresponding peculiarities, these may be transmitted. Obviously the cycle of functional activities involved in reproduction is instrumental to the transmission of any such peculiarities, or to their extinction, as the case may be.[1] These activities in themselves do not account for the genesis of variations of this kind, but only for the correlation of variations in the reproductive systems of the two sexes.

If the concept of physiological activity be thus broadened, as it should be, to include the *coördinated* activities of different organisms, whether of the same or of different species, it will be seen that the genesis of hereditary coadaptations between different organisms must be attributed in part to functional activity of a representative character. So much should be clear from our consideration of passive and inter-sex adaptations.

It may not be unprofitable to inquire how the materials—the discrete variations—for passive and inter-sex adaptations are provided. Many passive adaptations are so necessary, so fitted for the services which they perform, so closely correlated with other structures, that their origin can scarcely be attributed to chance variations and the preservation of these by natural selection. "Here belong, for example, the stratification of the lens in the human eye, the apodemes (inner projections of the chitinised cuticula) which protect the ventral nerve-cord of the crabs, the chitin hooks which hold together the fore and hind wings of many insects, and the similar structures which bind together the secondary branches of the feather vanes of birds." [2]

Functional activity in the ordinary sense of the term is manifestly not the originating cause of such structures. Neither could such struc-

[1] Rumpless hens furnish a good illustration of inter-sex maladaptation. Davenport states that such hens are sterile, owing to the fact that the erection of tail feathers is necessary to the exposure of the cloacal opening so as to permit the transfer of sperm. "Light Thrown by the Experimental Study of Heredity upon the Factors and Methods of Evolution," *Am. Nat.*, Vol. XLVI, 1912, pp. 136–137.

[2] Kellogg, V. L., *Darwinism To-day*, p. 207, quoting from Plate; Henry Holt and Company, publishers.

tures have originated through any of the processes postulated by anti-Lamarckian hypotheses, for they have explicit reference to other structures, and virtually the same criticisms would apply to anti-Lamarckian interpretations of these structures as to interpretations of specific active adaptations on the same hypotheses. The only hypothesis remaining is that developed in relation to the evolution of flowering plants so as to secure cross-pollination through the visits of insects, and to the extension of lung sacs in birds into the bones. We are obliged to conclude, from a comparison of the empirical consequences implied by the several hypotheses proposed for such cases, that organizatory agents have guided the genesis of these characters, but that these act on the basis of past interactions between the factors involved, and utilize available physical and chemical agencies in this process. And, as in cases previously analyzed, the activity of these agents must be conditioned and limited throughout by the physicochemical and functional factors associated with them.

We should interpret in a similar fashion passive adaptations not having such an explicit reference to specific features of the environment, but nevertheless of great utility to their possessors. Here would doubtless come lifeless cuticular structures associated with active responses, such as "knobs suited for crushing, saws suited for cutting, gimlets suited for boring, and so on." Many types of mimicry would be interpreted in the same way, and particularly those including active responses similar to those of the species imitated, such as the mimicry of the wasp by the beetle *Clytus arietis,* or of flies by certain hunting spiders, or of the berg-adder (*Vipera atropos*), a dangerous snake, by a harmless species of snake (*Dasypeltis scabra*).[1] Any tenable interpretation of such cases must lean rather heavily, it seems to us, on the assumption of organizatory agents capable of directing phylogenetic processes toward the remarkable adaptive characters in question. But any tenable interpretation must also assume that such agents operate on the basis of prior interactions among the factors involved, and that the entire process is limited and conditioned by the physicochemical agencies operative in the process.

We shall not undertake an analysis, from the standpoint of our general theory, of other types of passive adaptations, such as poisons, stings, protective coloration, etc. Doubtless many such adaptations, particularly those having little or no reference to specific features of the environment, would be explainable as the product of physicochemical factors, functional activity and morphological tendencies coöperating together, but without functional activity playing a

[1] Morgan, C. L., *Habit and Instinct,* p. 12.

representative rôle in the process, save indirectly, and without any very specific guidance of the process in the direction of adaptive results, by the organizatory agents operative in the process. There is apparently no theoretical difficulty in the way of assuming that variations of the sort issuing in such adaptations might originate in various ways, and that the combination of these variations into adaptive characters might be largely determined by "selective" factors in the environment.

One might work out a similar analysis for hereditary variations in the reproductive systems of reciprocally adapted sexes, but such an analysis would not yield any principles not already brought to light. Factors falling within all three of our basic categories would of course be involved in the genesis of hereditary variations in the reproductive system, but, in view of the rigorous selective tests to which any such variations are necessarily subjected, it seems unnecessary to suppose that the process of variation is guided in any strict sense by the organizatory agents involved in the process. These considerations, however, do not absolutely exclude the hypothesis that the process of variation is more or less rigorously controlled by those agents. Investigations could be executed which would throw a good deal of light on this problem, especially statistical studies to determine whether given individuals may not be fertile, in varying degrees, when mated with different individuals of the opposite sex. It may be that some evidence on this question has already been collected.

The interpretation of non-adaptive characters on our general theory may be briefly indicated. Such characters are widely distributed in nature. Indeed the majority of the characters that distinguish species from one another carry no demonstrable utility to their possessors.[1] For example, few of the characters distinguishing the various species of flatfishes from one another give the slightest indication of being useful to these several species,[2] while Kellogg and Bell have shown that the pattern-variates of the ladybird beetle, *Hippodamia convergens,* the proper identification and description of which require the use of a lens, cannot possibly be regarded as useful to their possessors.[3] Similar cases could be cited indefinitely.

Any valid genetic interpretation of such cases must allow, it seems to us, for factors coming within all of our basic categories. It is obvious that physicochemical processes and functional activity are concerned in their genesis, though we do not know enough about factors

[1] Kellogg, V. L., *op. cit.*, p. 38.

[2] Cunningham, J. T., "The Species, the Sex, and the Individual," *Nat. Sci. Vol.* XIII, 1898, p. 185; also, by the same author, *Hormones and Heredity*, pp. 22 ff.

[3] "Studies of Variation in Insects." *Proc. Wash. Acad. Sci.*, Vol. VI, 1904, pp. 203–332.

of these classes to define the specific rôles which they play in the process. An interpretation of such cases on the hypothesis of parallel induction would not be incompatible with our general theory, although, for reasons already stated, we doubt the validity of such an interpretation. On the other hand, we have no means of determining whether functional activity does or does not play a representative rôle in the genesis of such characters. A decision on these questions must necessarily be left to the future.

Nor are we able to define with any exactness the rôles played in these processes by morphological factors. We can say only that, insofar as such characters possess a definite organization different from the structure of the molecules or colloids constituting their material substratum, organizatory agents have determined these differences. For, as we hope to show in later chapters, physicochemical action never produces any but atomic, molecular, colloidal and crystalline structures, and such structures are not of the sort properly termed vital.

We have now considered as tests of our theory active adaptations presenting great difficulties to it, owing to the apparent exclusion of functional activity as a causal factor from their genesis; and have seen that these can all be interpreted consistently with the theory and the representative rôle which it assigns to functional activity in the genesis of such characters. We have shown, too, that the rival theories alleged to account for such cases meet with difficulties in this connection from which they cannot extricate themselves. In adapting our theory to these cases, it has been necessary, of course, to qualify the rôles played in the genetic process by functional activity and coördinate factors; this, however, does not represent an abandonment, but only a development, of the general theory which we have proposed. The application of our general theory to tropisms, passive adaptations and nonadaptive characters has served further to develop the theory, and possibly to throw some additional light on the genesis of those characters.

There remain to be considered two extremely important groups of adaptations, from whose genesis the Lamarckian factor, as ordinarily conceived, seems to have been excluded. We refer to certain types of instinctive behavior, and to complex inner structures with their many specific adaptations. To a consideration of the former we now turn.

CHAPTER VI

CRUCIAL PROBLEMS FOR THE ANALYSIS OF LAMARCKIAN AND ANTI-LAMARCKIAN HYPOTHESES (*continued*)

(3) Types of Instinctive Behavior from Whose Genesis the Lamarckian Factor Was Apparently Excluded

Before considering types of instinctive behavior from whose genesis functional activity of a representative character seems to have been excluded, some arguments against the Lamarckian hypothesis, drawn from a consideration of instinct in general, may be examined.

In a very acute analysis of conflicting hypotheses as to the origin of instinct, the late C. O. Whitman [1] rejected the Lamarckian interpretation, dubbed the "lapsed intelligence theory," on the ground that instinct is antecedent to intelligence in order of development, and is gradually superseded by the latter, as life evolves. Whitman asserted, in support of his position, that instincts are universal among animals, whereas intelligence is not; and that in the higher forms of life no case is known where intelligence has lapsed into instinct. He contended that "the primary roots of instinct reach back to the constitutional properties of protoplasm," and that specific instincts were evolved from those properties, not by way of habit, but through germinal variations and natural selection, assisted by "organic selection." Limitations of space prevent us from entering into the details of Whitman's argument and the experimental evidence cited in support of it, all of which are very valuable. Only his general argument can be examined here.

We may observe, first, that the general argument is of a formal nature, and scarcely touches the problem of the genetic relationships between specific instincts in the higher forms of life, and functional activity or "acquired characteristics." The assertion that no case among the higher forms of life is known where intelligence has lapsed into instinct verges toward a *petitio principii,* since the very question at issue is whether instinct has not evolved, in part, through intelligent behavior, or functional activity. Whitman is apparently appealing to

[1] "Animal Behavior," *Biological Lectures from the Marine Biological Laboratory, Wood's Holl, Mass.,* 1898.

the negative results of scientific observation and experiment relative to this question, but such results are by no means decisive, since much of the evidence bearing on evolutionary problems is of an inferential nature. The theories of descent and of natural selection were and are still largely based on evidence of this character.

Moreover, the general argument rests on a decidedly questionable assumption, and indeed one which Whitman himself virtually abandons in a later part of his monograph. This assumption is that instinct preceded intelligence in order of development and that the latter could not therefore have been a factor in the evolution of the former. But the researches of Jennings [1] and others have shown that there are no more grounds for the supposition that the so-called lower organisms are devoid of intelligence than that the so-called higher organisms are; while Whitman himself, after insisting on the contrast between intelligence and instinct, proceeds to argue that the distinction between them is one of degree not of kind,[2] and ends by concluding from "their dependence upon the same structural mechanisms" and from "their responsive adaptability," that there is a fundamental identity of the two. Other arguments in Whitman's paper relate essentially to the alleged improbability of a transmissionist mechanism, and these need not be examined here.

Whitman's basic argument, even were it sound, scarcely touches our position, for we have based our general theory on an analysis of hereditary adaptations to specific features of the environment, and have shown, we believe, that no valid interpretation of any such adaptation can exclude functional activity of a representative character as a causal factor in the process. Moreover, if instinctive behavior be defined as hereditarily determined responses to more or less specific features of the environment, it would be virtually impossible to discover or even to imagine a case of such behavior which did not originate as a result of interactions with that feature or those features of the environment to which it has reference. For an observation of any instinct in action, if the observation be complete, must necessarily include an identification of the object or situation to which it is a response; and since an instinct is, in part, a reflection or representation of its object or situation by the organism possessing the instinct, we cannot think of it as divorced, in its genesis, its existence or its action, from its object or situation. Indeed it would not be going too far to say that the environment to which an organism is by heredity adapted has literally impressed itself on the organism. This does not mean, of course, that particular objects or

[1] *Behavior of the Lower Organisms,* and other writings.
[2] *Op. cit.,* p. 330.

situations have impressed themselves upon the organism, but only that their species or genera have done so; or, speaking in terms of individuals, objects or situations reacted to in the past have impressed upon the organism certain elements or features common to them all, and common, also, to the present objects or situations to which the organism, as thus modified, reacts. In the lower as in higher forms of life instinctive adaptations often have reference to broad classes of stimuli or situations, and these may be later differentiated into adaptations having a more specific reference, but the same analysis applies, broadly speaking, to them all, and yields substantially the same conclusions.

The general theory which we have proposed, with the position it assigns to the Lamarckian principle, does not imply an interpretation of instinct as "lapsed intelligence." It contends only that functional activity in *some* representative sense must be involved in the genesis of specific instincts. It does imply, however, that in the genesis of many if not all instincts, organizatory agents are operative, and that these are guided in some sense by past functional activity related to the objects or situations of those instincts. While this seems very much like action on the basis of intelligence, being guided, as it is, by a species of experience, there is no necessity for supposing that this intelligence, if it be such, is similar to human intelligence, or to intelligence manifested in the behavior of any organism as a whole, whatever the species to which it belongs. Nothing in the facts of behavior are inconsistent with the hypothesis that intelligence of the former sort, if we can call it that, is characteristic of even the lowliest organisms, while the genesis of instincts and other hereditary characters cannot be adequately accounted for on any hypothesis denying such intelligence, or organizatory processes guided by experience or functional activity.

Nevertheless there are types of instinctive behavior which have presented great difficulties to every type of genetic theory which has been applied to them, and particularly to the Lamarckian theory. The most difficult cases of this sort are found among the insects, and we may employ some specially difficult instances thereof as tests of our theory. We may examine first in this connection the reproductive behavior of the Yucca moth. We have already considered the adaptations of the Yucca plant *to* this insect, considered as an indispensable agency in the fertilization of the former. It only remains to examine the other side of the reciprocal relationships between the two species.[1]

[1] The case is fully described in Kerner, A., *Natural History of Plants*, translated by Oliver, F. W., Vol. II, pp. 156–159. A convenient summary of the facts in the case is given in Morgan, C. L., *Habit and Instinct*, pp. 13–15.

The moths emerge from their chrysalis-cases just at the time the flowers of the Yucca open; the female moth proceeds to collect pollen from the anthers of one flower, kneads it into a pellet, seeks another flower, pierces with her ovipositor the tissue of the pistil, lays her eggs among the ovules, and then stuffs the pellet of pollen into the funnel-shaped opening of the stigma. It has been proved that without this behavior on the part of the moth, the ovules of the plant are not fertilized; and that the fertilization of the ovules is necessary to the development of the moth's larvae, since these feed only on the developing ovules. Not all the latter are consumed, however, and thus the reproduction of both species is achieved.

Our interpretation of the moth's adaptation to the Yucca plant corresponds at all points to our previous interpretation of the reverse relationship. Intelligence in any anthropomorphic sense is clearly excluded from the moth's behavior, for it does not experience the adaptive results of its behavior. Nor is imitation a factor in its behavior. The possibility is not excluded, however, that this cycle of reproductive behavior was repeated one or more times by individual ancestors of the moth, nor that those ancestors had some experience of the results of their behavior. But it is difficult to suppose that the moth's present instincts are the product of intelligent behavior and habit, in the ordinary sense of those terms.

Our previous analysis of the germinal interpretation of active adaptations applies with full force to the present case. The probability of such adaptations arising through chance variations in the germ-plasm (whether interpreted chemically or non-chemically) is practically nil, and, indeed, unthinkable, for the Yucca flower, a specific feature of the environment, has literally been impressed upon the organization of the Yucca moth, an impossibility without the presence and influence of the Yucca flower itself, unless we postulate the agency of a providential power interested and intelligent enough to accomplish such a result. And, as we saw, anti-Lamarckian hypotheses which do assume the presence and influence of the environment in the genesis of adaptations thereto meet with insuperable difficulties in their attempts to interpret such cases.

The Lamarckian principle must therefore be retained, but, as in the evolution of flowering plants, a specially important rôle in the genetic process must be assigned to organizatory agents operative therein. There seems to be no escape from this conclusion. Either such processes are under the guidance of external agents interested and powerful enough, and possessed of the requisite knowledge (and a knowledge *not* derived from experience) to establish such adaptations—a con-

sequence implied by all the anti-Lamarckian hypotheses; or they are under the guidance of entelechian agents not external to the process, but which do not act on the basis of prior experience or functional activity—as certain vitalistic theories hold; or they are under the guidance of entelechian or organizatory agents which do act on the basis of experience or functional activity. The assumptions and implications of the latter hypothesis are, we submit, more consonant with the whole body of our experience, and particularly with the established results of our science, than are the assumptions and implications of any rival hypothesis.

The same analysis applies to similar cases, be they more or less complicated. Indeed we may say that the more complicated the series of instinctive acts essential to reproductive or other ends of the organism, the more cogent becomes our theory in its interpretation thereof, and the less tenable any rival theory. The reproductive behavior of *Sitaris* will support this contention. The mother laying her eggs at the entrance of the gallery excavated by *Anthophora;* passage of the larvæ, much later, to the drones of the latter as they emerge from the gallery; transfer, subsequently, to the queen-bee during the nuptial flight; then from her to the eggs she lays, which serves both as food and as a support in the honey sealed up with the eggs by the queen-bee; later feasting upon the honey, etc.: a number of adaptations to specific features of the environment, which must function in a fixed manner and order, if the whole process is to accomplish its purpose. In view of our previous analysis, is it not clear that those features of the environment and interactions therewith must have been causal factors in the genesis of this complex adaptation, and that the elements of the adaptation, if we may so speak, were organized into a highly effective behavior complex by some agency or agencies endowed with a capacity for this organizatory work? Either this must be the conclusion, or we must appeal to a guiding providence, or to a knowledge on the part of agencies at work in the organism, which was not derived from experience, and with which they must therefore have been miraculously endowed.

The last case to be considered under the present caption is that of workers' instincts in the social Hymenoptera. Evidence from this source was regarded by Weismann and his followers as definitely overthrowing the Lamarckian hypothesis, although the Lamarckians themselves vigorously combated this interpretation of the evidence. It was conceded by Weismann that the workers in most species of this group occasionally lay eggs from which fertile males are produced, but he contended that this occurred too rarely to account for the wide dis-

tribution of workers' instincts and structural preculiarities. Those characters were due, instead, to the occurrence of chance variations in the germ-plasm of the queens and drones, which were useful and hence perpetuated by selection. These variations issued, of course, into the neuter castes.

Spencer contended, in rebuttal of this argument, that the workers' instincts arose in the presocial and semi-social states of the social Hymenoptera's ancestors, and that differences in nutrition of larvæ determine development into fertile and infertile females, respectively, with the division of labor and development of different instincts correlated therewith. This interpretation encounters a serious difficulty in the highly adaptive character of the workers' instincts, which could scarcely be accounted for by differences in nutrition alone. Emery has proposed a hypothesis agreeing in the main with that of Spencer, but resting on a broader foundation. According to his hypothesis, differentiation into distinct castes, while having a germinal basis, is proximately due to developmental causes embracing a variety of trophic stimuli.[1]

We shall largely limit ourselves, in dealing with this group of phenomena, to a consideration of polymorphism in ants, the more highly evolved of the social Hymenoptera, and particularly to features thereof which have a direct bearing on our problems.[2]

The evidence taken as a whole tends to support the hypothesis that morphological differentiation in ants, while undoubtedly having a germinal basis, is due to sundry developmental causes, among which quantity of food is by far the most important. The social Hymenoptera are characterized by a highly plastic type of ontogeny, capable of being guided in the various channels actually exhibited by the several species thereof, but not predetermined in the egg itself, excepting as to sex, the latter depending in practically all cases on fertilization or non-fertilization of the egg.

The more important evidence in support of this view may be briefly indicated. While no direct causal connection between feeding and ontogenetic loss or development of characters has been established, the indirect evidence supports the hypothesis that there is an intimate causal relationship between the two. It is probable, in the first place, that all worker ants when supplied with abundant food develop a capacity for laying eggs, and the males derived from such eggs are per-

[1] Wheeler, W. M., *Ants, Their Structure, Development and Behavior*, pp. 100-101.

[2] The principal sources for the facts cited are Wheeler, W. M., "The Polymorphism of Ants," *Bull. Amer. Mus. Nat. Hist.*, Vol XXIII, 1907, pp. 1–93; and, by the same author, *Ants, Their Structure, Development and Behavior*, 1910, Chaps. VI and VII.

fectly normal, and capable, therefore, of fertilizing the queens. Moreover, numerous observations show that diminution in the size of perfect females and, in pathological cases, reversion to the worker type may be due to underfeeding. All variations in size, which are so characteristic of some species, and which figured conspicuously in Weismann's writings on the subject, may be confidently put down to the same cause. The complete disappearance of the worker caste in several parasitic genera (*Anergates, Wheeleriella,* etc.), and the dwindling of the caste in other species (e. g., *Polyergus rufescens*) must be similarly explained, that is by abundance of food. Favoring the broader developmental hypothesis proposed by Emery are certain modifications produced by the parasitic condition, either in the host or in the parasite itself, as the case may be.

While it is not unlikely that other factors than those indicated may influence the ontogenies of various ant species, enough has been said to show that the assumption of different kinds of "ids" or "determinants" is not essential to an explanation of polymorphism in this group. The adaptive instincts of the worker, which may be presumed to have arisen since the social state with its distinct castes was established, are most simply explained by the hypothesis that there has been an influx of workers' germ-plasm in the given species.[1] Fertile workers, according to Wheeler, are much more common among all groups of social insects than has generally been supposed. Although this hypothesis should be subjected to experimental tests,[2] it is certainly not incompatible with present evidence.

Not only are the known facts regarding the social Hymenoptera compatible with a Lamarckian interpretation of workers' characteristics, but some competent authorities at least believe that interpretation is more consonant with the facts than is the selectionist interpretation. While the question cannot of course be settled by an appeal to authority, it suffices for our present purpose to show that nothing in the evidence available to us excludes the hypothesis that functional activity has been a factor in the genesis of the instinctive adaptations peculiar to the worker castes, and of other active adaptations implied by the structural characteristics of those castes.

We venture to suggest that an analogy helpful in the consideration of this problem might be drawn from the ontogenesis of human individuals. The mental development of the child is largely determined, as every one knows, by the language, customs, ideas, arts and other cultural controls of the group to which it belongs. The end products

[1] Wheeler, W. M., *Ants*, etc., p. 116.
[2] *Op. cit.*, p. 117.

of the developmental processes so guided are indefinitely diverse, as a comparison of the speech habits, manners and beliefs of Englishmen, Chinese, Indians, etc., clearly demonstrates. These differentiations are quite as extensive and profound as are the caste differentiations among the social Hymenoptera. The differentiations in the two cases are quite dissimilar, of course, and are produced by factors of dissimilar kinds, but both types of processes can be subsumed under the same general category. And, what concerns us here, we have evidence that there is a plasticity in the hereditary endowment of the social Hymenoptera, and developmental factors capable of moulding that plastic material into very different forms, just as there are such factors and such plasticity of endowment in human development.

An additional suggestion may be hazarded. The facts of cell-division, as these have been discovered in recent years, would seem to render extremely improbable the qualitative differentiations of "determinants" in the germ-plasm of the queen, and combinations thereof requisite to the production of two or more distinct castes, that Weismann's theory postulates. That would imply a type of cell-division, maturation and chromosomal behavior such as cytologists have not yet discovered or felt justified in inferring. But this problem is worthy of investigation on its own account, if, indeed, such investigations have not already been initiated.

We venture to assert, on the basis of these considerations, that none of the facts regarding the development of the social Hymenoptera invalidate our analysis of hereditary active adaptations as applied to the instinctive behavior and morphological characteristics of the worker castes, but that, on the contrary, the evidence taken as a whole, when cautiously interpreted, supports that analysis. We may say, on the other hand, that our analysis strengthens the Lamarckian interpretation of the empirical evidence in the case.

The differentiation of castes among the bees parallels in the main the corresponding phenomena among ants. One of the principal differences between the two is that quality but not quantity of food plays an important rôle in ontogenetic development among the bees. But this does not affect the problem of the origin of the workers' instincts. Only in one species of honey-bee (*Apis mellifica*) are workers entirely excluded from the reproductive circle, and it is said that egg-laying workers are not unknown among the Egyptian variety of this species (*A. m. fasciata*). This, added to the fact that, in the differentiation of instincts as between queens and workers, the principal changes have taken place in the former, together with the possibility that certain types of behavior on the part of the worker believed to be instinctive

are, in reality, induced by their social environment,[1] permits a Lamarckian interpretation of workers' instincts in the one case where the differentiation of the worker caste seems complete.[2]

The highly adaptive distribution of instincts and morphological characteristics in the social Hymenoptera cannot be wholly accounted for, however, without assuming that the organizatory agents operative in their genesis have played a specially important rôle therein. The adaptive division of instincts between queens and workers, the infusion of workers' germ-plasm in the species, and thereby the addition to the species, of instincts developed in the worker caste, the evocation of workers' instincts in the queens when necessity arises, as in the founding of new colonies, could not be wholly accounted for by differences in nutrition or other developmental influences. We have represented in these phenomena a highly complex and adaptive organization of a species as a whole, and organization of this character is the achievement of organizatory agents sufficiently qualified for this work, and not the product of physicochemical or of Lamarckian factors alone, or of both sets of factors combined. Both have been operative, and have conditioned and facilitated the organizatory result, but to them must be added as a coördinate factor the activities of the organizatory agents themselves.

The types of instinctive behavior now considered as presenting difficulties to our theory, owing to the apparent exclusion of functional activity from a share in their genesis, may be taken as sufficiently representative of other cases which might be adduced against the theory, or rather as presenting difficulties quite as serious as any other cases which could be brought forward. If the theory can weather difficulties of the sort we have considered, it may be regarded as impregnable to equal or lesser difficulties which might be opposed to it.

(4) Complex Inner Structures Apparently not Susceptible of a Lamarckian Interpretation

Difficulties presented to our theory by complex inner structures have been dealt with, by implication, in our analysis of intra-organic adaptations and need not be considered anew.

The case of the retina, which presents some specially difficult prob-

[1] Cunningham, J. T., "Neuter Insects and Darwinism," *Nat. Sci.,* Vol. IV, 1894, pp. 281–289.

[2] A résumé of the evidence regarding the origin of caste distinctions among the bees, with references to the literature, is given in Semon, R. *Das Problem der Vererbung "erworbener Eigenschaften,"* pp. 22–23; and *The Mneme,* English translation by Louis Simon, pp. 221–227.

lems, can be more conveniently treated in connection with our examination of the vitalistic theory proposed by Henri Bergson, wherein a good deal of significance is attached to this case.

(5) Psycholamarckism

Psycholomarckism is a theory of life which stresses the rôle of mental factors therein and correspondingly minimizes the part played by material or mechanical factors. The doctrine is designated psycholamarckism, or neolamarckism, because it is founded on, and represents an elaboration of, views attributed to Lamarck which stressed mental factors in vital phenomena, but which had not been incorporated in current Lamarckian doctrines. Cope was one of the earliest and ablest exponents of this theory, but it has received its greatest elaboration at the hands of certain German biologists.[1] Our examination of the theory will have special reference to Pauly's exposition, generally conceded to be the best formulation we have of it; but attention will be given to significant theoretical differences between leading representatives of the general doctrine. Our résumé and criticism of the doctrine will be brief, and presented from the viewpoint of the general theory developed in the course of the previous discussion.

Psycholamarckism differs from other types of Lamarckism, by its acceptance (and expansion) of Lamarck's view that "the production of a new organ in an animal body results from the supervention of a new want (besoin) which continues to make itself felt, and of a new movement which this want gives rise to and maintains." [2] The theory includes, in common with other types of Lamarckism, the hypothesis that many functionally produced modifications are inherited. While it is doubtful whether Lamarck himself conceived wants and the movements initiated thereby in terms of desires and volitions,[3] the psycholamarckians do interpret them in this sense, maintaining that *all* vital processes are teleological in nature and are characterized by, or at least have their origin in, sensation, volition and other mental processes. In taking this position, they are obviously giving to Lamarck's doctrine a much wider application than he himself claimed for it.

[1] Cope, E. D., *The Origin of the Fittest; The Primary Factors of Organic Evolution;* Pauly, A., *Darwinismus und Lamarckismus;* Schneider, K. C., *Vitalismus, elementare Lebensfunktionen;* Wagner, A., *Der neue Kurs in der Biologie. allg. Erörterungen zur prinzipiellen Rechtfertigung der Lamarckschen Entwicklungslehre;* and other works by various authors.

[2] *Animaux sans Vertèbres,* Introduction, translation in Packard, A. S., *Lamarck, His Life and Work,* p. 346. Substantially the same principle is propounded in the *Philosophie zoologique;* see Packard, *op. cit.,* p. 302.

[3] Packard, A. S., *op. cit.,* pp. 350–352.

The basic contention of the psycholamarckians is that life in all its manifestations is of a teleological character. The teleological action of the organism is not determined from the outside, but, in Wagner's phrase, is autoteleological. It consists in an active synthesis of two experiences, one the experience of the need, the other, of the means to its satisfaction, the synthesis itself being effected through judgment, or a determination of the means requisite to the satisfaction of the given need.[1]

While this concept is variously formulated by different psycholamarckians, the differences between them on this point are not fundamental. Schneider holds that three fundamental psychic factors are operative in every vital process: (1) sensation, (2) feeling and (3) will. The first is correlated with an external stimulus, the third with an application of energy, the second, which is the central feature of every psychical process, with sensation and will.[2] Wagner expresses the same general concept in the statement that psychical factors are mediatory between stimuli from, and reactions to, the environment.[3] Psychical or teleological action comprehends in one causal process the stimulus to action, the means to action, and the state of the organism as modified by the given action.[4]

Teleological action thus defined characterizes all vital processes whatsoever. Artistic construction by man is the highest type of teleological action, and there are numerous gradations between this and the simplest type of action at the lower end of the scale; but the behavior of plants and of the lowest animals is determined by causes analogous to those operative in man's behavior. Only simple acts of thought are involved in the behavior of these lower organisms, as their cells act directly upon one another; yet every cell of a plant knows and acts with reference to the experience of the whole.[5] There is, throughout, the coordination of inner conditions in the organism, which is the presupposition of all organic teleology, and the synthesis of diverse perceptions, feelings, etc., which is a condition of all judgment and hence of all teleological action.[6]

Phylogenetic changes all originate in the needs of the organism and the utilization of means to their satisfaction. The course of evolution is not determined according to any general laws, nor guided by natural selection, but represents the summation of specific modifica-

[1] Pauly, *op. cit.*, p. 8.
[2] Schneider, *op. cit.*, p. v.
[3] Wagner, *op. cit.*, p. 61.
[4] *Ibid.*, p. 79.
[5] Pauly, pp. 165–167, 172, 183.
[6] *Ibid.*, p. 163.

tions induced by new needs, due to changed conditions of the environment. Degeneration and arrested development are interpreted in a similar fashion. These are determined by the cessation, diminution or continuance of functional activity, as the case may be. [1]

In the exposition of these views there is much drastic criticism of mechanistic theories of life, but the rôle played by physical and chemical factors in vital processes is recognized. The concept of the direct influence of environment represents the conditions but not the causes of phylogenetic changes, although such changes are partially dependent on those conditions.[2] The organism utilizes any means that may be available in the satisfaction of its needs, but the nature of such means has no predetermined relation to the need. And the technical consequences of the means employed, that is the specific structural modifications resulting therefrom, may be farreaching.[3] Similar organs are produced by the similarity of needs and conditions, but not otherwise. Mechanical arrangements in the body are really the products of teleological action, and the limited influence of functional activity on structure is also an evidence of teleology. In brief, all vital processes are teleological in nature and cannot be interpreted in mechanistic terms. Still, the material conditions which give rise to new needs or serve as means to the satisfaction of needs partially determine functional and morphological changes. The more significant factors in such changes, however, are the psychological, for these factors guide and dominate the processes of change.

The metaphysical assumptions of the theory are of special interest to our inquiry. The general position is that life must be explained in terms of matter and energy, but that these must be endowed with psychological properties. Pauly maintains that nothing new in principle intervenes in the evolution of the organic from the inorganic, but psychological properties, not recognized by physics and chemistry, must be added to the inorganic in order to account for organic teleology. The teleological or purposeful is the criterion of the psychological, the latter manifesting itself subjectively as sensation, reflection and volition, or objectively, as the employment of means in the attainment of an end.[4] The teleological is manifested in the inorganic as energetical processes, and, conversely, psychical processes in the organism represent the application of energy. Life is erected into a universal category and made the active factor in our world substance.[5]

[1] Pauly, pp. 20–22.
[2] Pauly, pp. 24, 109.
[3] *Ibid.*, pp. 113–114, 118, 125.
[4] Pauly, pp. 144–145.
[5] Pauly, pp. 163, 166.

Schneider's metaphysical assumptions are much the same. He accepts paleovitalism as a general theory of life, and emphasizes its assumption of a special vital or psychical energy as activating all living substances. There is, however, no difference in principle between vital and other forms of energy; it is differentiated from the latter, in the main, by its greater complexity.[1] Neovitalism lacks a sound basis, because it does not assume a special energy of this character.[2] Schneider assumes, in addition, the existence of biomolecules, ergatids and other atomistic units of living matter. These are all interpreted in psychological terms, however, with sensation, feeling and will as the fundamental elements therein. All existence is constituted of these three elements, and there can be nothing unconscious in the world.[3]

Wagner, while adhering, in the main, to the views of Pauly, favors a more thoroughgoing psychological conception of reality than does the latter, and rejects the atomistic assumptions of Schneider [4] as inconsistent with the psychological interpretaion of life.

Summarizing, we may say that the central doctrines of psycholamarckism are (1) its teleological conception of vital processes; and (2) its monistic psychological conception of reality in general. The first doctrine is a scientific hypothesis, the second a metaphysical theory, upon which this hypothesis is based. An important corollary of the second doctrine is the proposition that the categories of factors in organic and inorganic phenomena are fundamentally the same.

We may say at once that, in our judgment, the scientific hypothesis in question is valid, as a general proposition, but that it requires a number of important qualifications; and that the metaphysical assumption combined with that hypothesis is not a valid one, and not compatible with the facts interpreted on the hypothesis.

If we hold, with Pauly, that the teleological or purposive is the criterion of the mental, then we must agree that vital processes are to be partially interpreted in terms of the mental. Objections might be urged against this criterion. When viewed from the outside, all types of vital processes are apparently the same in kind as the mental processes of which we know most, that is, our own mental processes. But whether the former are accompanied by any such states of awareness or consciousness as accompany many of our mental processes, there is no means of knowing. Opinions differ regarding the significance of consciousness for the mental in our own case, as well as for

[1] Schnieder, *op. cit.*, pp. iv,v.
[2] *Ibid.*, p. 1.
[3] *Ibid.*, p. v.
[4] Wagner, *op. cit.*, p. 89.

the mental in general; and one's attitude toward the psycholamarckian view respecting the relation of the mental to the vital will be affected by his opinion on that point.

The facts summarized by the psycholamarckian doctrine of organic teleology are much the same facts as those we have subsumed under the category of organizatory processes. We have argued that organizatory agents are operative in all vital processes, processes that overstep the limits of the physicochemical; and we may add, what has largely remained implicit hitherto, that such processes are largely, though not completely, teleological in character. Their tendency, as a general rule, is to maintain the organism and readjust it continuously to the changing conditions of its existence, where changes necessitate such modifications. This being the general tendency, we are compelled to admit that vital processes are, as a rule, of a teleological character.

But the psycholamarckians tend to overestimate what may be called the teleological capacity of the organism,[1] and do not allow sufficiently for the limitations imposed on such capacity by physicochemical conditions, functional activity and the organizatory agents themselves.[2] Instances without number there are, where species have suffered extinction, setbacks or serious maladaptations, because of difficulties or limitations imposed by one or more of these factors. Teleological capacity must be regarded as relative and limited—relative to the milieu and its changes, and limited by the physicochemical, functional and morphogenetic properties of the organism.

This criticism does not overlook the view of Pauly and others that the action of the organism and its consequent modification are contingent on the environmental conditions to which it must adapt itself. The point is that the theory underestimates the causal rôle played by such conditions, and overestimates the causal rôle played by the teleological capacities of the organism. We can find no warrant for Driesch's contention that *too much* weight is given by the theory to contingent variations in the organism, for it certainly endows living matter with sufficient organizatory capacities to account for the types of organization actually manifested by living organisms.[3] We shall side with Driesch, however, in his view that the organizatory capacities of the organism cannot be attributed to the physicochemical substances combined therein. On the other hand, critics like Plate do not take into account the weight given by the psycholamarckians to the material

[1] Cf. Plate, L., *Selektionsprinzip,* 1913 ed., pp. 589, 595.

[2] It should be stated that some psycholamarckians, including Prochnow, E. Becher and S. Becher, are less extreme in their views respecting the teleological capacities of the organism. See Plate, L., *op. cit.,* p. 590.

[3] Driesch, H., *Science and Philosophy of the Organism,* Vol. I, pp. 271 ff.

conditions of the organism, both inner and outer, and accept a narrow traditional view of the mental in rejecting, without serious examination, the psychological interpretation of vital processes.[1]

The principle defects of the theory, however, are to be found in its metaphysical assumptions. By endowing all matter with similar psychological properties, the theory renders itself incapable of accounting for the differences between the organic and the inorganic. Even should we accept the proposition that matter in its inorganic state is endowed with psychological properties, that would give us no aid in accounting for vital processes which overstep the limits of physicochemical action. For vital processes represent a type of action not exhibited by inorganic substances, however completely we endow the latter with psychological properties. No type of physicochemical action is known which would account for the morphogenetic processes of the organism, and there must be a type of factor operative in the latter, therefore, which is absent from the inorganic. No idealistic metaphysics can dissolve the distinction between the two. Nor can organic activity be resolved by any system of metaphysics into the physicochemical alone, so that, to put the matter negatively, we are obliged to infer the existence and operation, in the organic, of nonphysicochemical factors. These positions it will be our business to make good in the chapters to follow. The psycholamarckians, by rejecting the materialistic conception of life and at the same time redefining matter to include psychological properties, have erected assumptions for their theory which lend no support whatever to their teleological interpretation of life processes. But a reform of the theory along the lines indicated by our criticism would represent a virtual dissolution of the theory, for it would have been taken up into a broader synthesis where only its teleological interpretation found a place, and that interpretation would not have been contributed, but only emphasized, by the theory.

(6) Problem of a Transmissionist Mechanism

The "inconceivability" of a transmissionist mechanism used to figure rather conspicuously in anti-Lamarckian polemics on problems of phylogenesis. But it is now generally agreed that arguments from this alleged inconceivability are no longer valid. These arguments were based on the supposed segregation and continuity of the germ-plasm, and with the overthrow of this doctrine and the demonstration of various functional interrelationships between gonads and other tissues of

[1] See Plate, L., *op. cit.*, p. 589.

the body, several possibilities of a transmissionist mechanism [1] were opened up. But little has been done as yet toward identifying processes whereby functionally produced modifications might be transmitted to offspring, and little would be gained for our purposes, therefore, by a survey of the evidence on the subject.

Many hypotheses have of course been proposed to show how somatic modifications might affect the germ cells. Darwin's gemmules, Spencer's physiological units and various other hypothetical carriers of such modifications come under this head. Plate accepts Weismann's theory of determinants as indicating a satisfactory mechanism for the transmission of acquired characteristics, pointing out that the assumed presence of determinants in the somatic tissues, as well as in the germ cells, renders this application of the theory possible.[2] O. Hertwig has argued that all the tissues of the body are intimately dependent upon one another, and that modifications induced in one tissue could readily affect all other tissues, including the germ cells. He believes that from this point of view the inheritance of acquired characters might be accounted for.[3] Haacke takes a similar position. The effects of external influences on the organism, according to his view, are so complex and farreaching that a character developed in response thereto must or may affect the germ-plasm.[4]

The mnemonic theory of heredity has been especially popular. Haeckel, Cope, Hering, Samuel Butler and many others have proposed or accepted theories of this general type. Semon has recently elaborated a theory which brings under the mnemonic principle all phenomena of organic reproduction, including habit, memory, heredity, regeneration, etc. Semon's version of the mnemonic hypothesis will receive more detailed consideration later on.

Chemical theories of heredity allowing for the Lamarckian principle have also been advocated. Here come hypotheses proposed by Delage, Vernon, Schiefferdecker, MacBride, Adami, Cunningham and Guyer.[5] The views of Cunningham and Guyer on this problem will be presented later.

Many other theories allowing for the Lamarckian factor in phylo-

[1] The term mechanism is here used in a descriptive sense only, and not as implying a mechanistic interpretation of the processes so designated.

[2] *Selectionsprinzip,* 1908 ed., pp. 328 ff.

[3] *Das Werden der Organismen,* 3d ed., pp. 571 ff.

[4] Haacke, W., *Grundriss der Entwicklungsmechanik,* pp. 289 ff. Cited by Kellogg, V. L., *Darwinism To-day,* pp. 270–271.

[5] See Cunningham, J. T., *Hormones and Heredity,* Introduction. This author makes no reference to Guyer's work on the problem.

genesis could be cited, but the references already given will suffice to show that a transmissionist mechanism is conceivable, at least to many competent biologists who have tackled the problem. And some progress has been made, as we shall see, toward an identification of specific processes involved in the transmission of somatic modifications.

We might leave this problem without more ado, since in the present state of our knowledge, it carries no adverse implications for the Lamarckian hypothesis. So long as there are various untested possibilities of a transmissionist mechanism, no valid argument can be drawn from the failure hitherto actually to identify such a mechanism. This fact simply means that the connection between somatic and hereditary modifications (granting such a connection) is at present inexplicable. Science is replete with such inexplicable connections. And, however far we pursue our researches into the phenomena of nature, we must always halt at connections between phenomena which are quite inexplicable. If a given connection be accounted for by later researches, some other inexplicable connection or connections will take its place, and so on *ad infinitum.* Could we suppose that we had penetrated, by our observations and inferences, to absolutely simple connections, these would in their turn be altogether inexplicable. The inexplicable in nature, or the inconceivable, if you will, always has the last word. We can push it back further and further, and this is *the* task of science, but the more elementary connections in nature must ever appear to us inexplicable.

Any inexplicable mechanism is, then, either a connection or association of phenomena, which is elementary and cannot be explained by anything more elementary, or it is a mechanism which has more elementary mechanisms underlying it, but which have not yet been discovered. If transmission of somatic modifications be proved to occur, we must infer that a mechanism of transmission exists. A task of science will then be to reduce this mechanism, if possible, to the terms of simpler mechanisms. Whether it succeeds or fails in so doing, there is created no added presumption for or against the existence of the mechanism inferred. We may justly claim that, if we have demonstrated that functional activity is a causal factor in the genesis of hereditary characters, the existence of a mechanism requisite thereto has also been demonstrated.

We might, as aforesaid, leave the problem at this point, but it should be of interest to examine mnemonic and chemical theories of heredity allowing for the Lamarckian factor, in order to determine whether they might account for the transmission of somatogenic characters. We

shall limit ourselves, in this connection, to the most recent versions of those theories, beginning with Semon's theory of the mneme.[1]

Semon regards memory, habit and heredity as manifestations of a common property, which he designates the *mneme* or *mnemic* principle. The mnemic property manifested in these various phenomena must be traced back to the irritability of organic substance, and its capacity for retaining some more or less permanent effect of its reaction to stimulus. Such an after-effect is termed an engram, and the effect of a particular stimulus on a particular substance is designated its engraphic effect. Engraphic effects are not limited to the individual reacting to the given stimuli but may appear in offspring as hereditary engraphic modifications. Mnemonic phenomena differ from repetitions of inorganic phenomena in that they are not dependent on a complete duplication of the originating causal conditions. An engram may be activated either by stimuli virtually equivalent to those initiating the engram, or by stimuli qualitatively or quantitatively different therefrom.

Further details of Semon's theory and of the elaborate terminology he employs in its exposition may be omitted. It should be added, however, that he interprets the organism and its environment in terms of matter and energy. The author often speaks of the stimulus or situation to which the organism reacts as an "energetic condition." However, he does not explicitly interpret habit, heredity and other reproductive processes in physical and chemical terms, recognizing that data for such an interpretation are not available.

Detailed comment on this hypothesis will, for our purposes, be unnecessary. Semon was a convinced believer in the Lamarckian hypothesis, and his theory of the mneme represents an attempt to subsume under a single category the various types of hereditary characteristics to which he would apply that hypothesis, together with phenomena of individual development deemed to be similar, in principle, to those characteristics. In *The Mneme* and other works Semon attempted to demonstrate the applicability of the Lamarckian hypothesis to those characteristics. Unquestionably he has in these works advanced the discussion of evolutionary problems, and done much to rescue the Lamarckian hypothesis from the discredit into which it had fallen.

But the theory of the mneme does not solve the problem of a transmissionist mechanism. It is only a restatement of the facts to be accounted for, assuming those facts to have been established. Stated differently, the mneme is a descriptive category only, and not a princi-

[1] *Die Mneme als erhaltendes Prinzip im Wechsel des organischen Geschehens,* third edition; translated into English by Louis Simon as *The Mneme,* 1921.

ple of explanation. The experimental evidence adduced in support of the theory is of value, but it would have the same value without that particular interpretation being annexed to it. And Semon's argument demonstrates nothing which had not been recognized by other investigators respecting the similarities between memory and habit, on the one hand, and heredity, regeneration, etc., on the other.

Whether the *factors* in those phenomena are similar is the very question in dispute between Lamarckians and anti-Lamarckians. In dealing with that problem Semon proceeded upon the same assumptions and employed the same methods as his opponents. They all assumed that it was possible, in principle, to divide the organism into independent systems—germ-plasm, soma, etc.—and to isolate from one another specific processes of possible change therein, such as parallel induction, somatogenesis, etc. These assumptions and methods have largely vitiated their investigation of fundamental problems, and made it impossible for them to get beyond a superficial eclecticism and work out a really organic theory of the genesis of hereditary modifications.

Nevertheless, the subsumption under the same descriptive category, of habit, memory, heredity and other phenomena of organic reproduction lends support to a fundamental assumption of the Lamarckian hypothesis, or of any more synthetic theory, such as we have proposed, to which it may be subordinated. This is the assumption that fundamentally the same principle is at work in phylogeny as in ontogeny and, consequently, that a theory incorporating that principle is logically superior to one denying it. And yet, since that assumption is called in question by the anti-Lamarckian theories of phylogenesis, its validity has to be vindicated anew. The only way in which this can be done conclusively is to compare the implied empirical consequences of Lamarckian and anti-Lamarckian hypotheses, respectively, in order to determine which of them harmonizes best with the whole body of our experience.

Two recent formulations of the chemical theory of heredity will now be considered. We shall take up first Cunningham's theory respecting the relation of hormones to heredity.[1]

This theory assumes that adaptive and non-adaptive characters are distinct from a genetic point of view, the adaptive characters being somatogenetic, and the non-adaptive gametogenetic, in origin. Non-adaptive characters conform to the mutationist and Mendelian conceptions respecting the origin and transmission of hereditary char-

[1] *Hormones and Heredity*, 1921; "The Heredity of Secondary Sexual Characters, in Relation to Hormones, a Theory of the Heredity of Somatogenic Characters," *Arch. f. Entwm.*, Vol. XXVI, 1908, pp. 372–428.

acters, whereas adaptive characters do not conform to these conceptions, but originate in somatic functional activity and are impressed on the germinal material by means of hormones liberated through such activity. It is argued that secondary sexual characters, which correspond in their development with the development and functional activity of the gonads, are of an adaptive nature, sharing in the reproductive function as a whole; and that since it has been proved that these characters are influenced by the gonads, through hormones or internal secretions, it is reasonable to suppose that there is an influence of a reverse order, that is, that hormones or internal secretions, stimulated presumably by functional activity, have given rise to secondary sexual as well as other adaptive characters. The argument is clinched by the proposition that "the facts concerning the action of hormones are beyond the scope of current conceptions of the action of factors or genes localized in the gametes and particularly in the chromosomes. According to these conceptions, characters are determined entirely by the genes in the chromosomes, whereas in certain cases the development of organs or characters depends on a chemical substance secreted in some distant part of the body."

Into the details of this discussion we shall not enter, but limit ourselves to an examination of the general argument in support of the main hypothesis. Cunningham apparently assumes that characters which develop late in the course of ontogeny are not ultimately determined by genes or factors localized in the gametes. Secondary sexual characters are not determined, according to this hypothesis, by factors in the germ cells, but by secretions or hormones liberated by the gonads. A simple logical consideration will show that this hypothesis is of a onesided character. While it is doubtless true that secretions from the gonads partially determine the development of secondary sexual characters, the gonads themselves and their secretions are ultimately determined by the factor or factors in the gametes which determine sex. Other factors are of course involved in the development of these characters, for they affect many parts of the body, and the genes or factors for those parts are therefore operative in their development. We may generalize this observation by the statement that all types of chemical action characteristic of a given species, including all types of internal secretions, are ultimately determined by hereditary factors in the gametes, however those factors be conceived.

How the germinal factors themselves originate is of course a different question, and Cunningham's treatment of this question is less open to criticism, though he does not appear to have proved the cor-

rectness of his answer to it. Considerable evidence is adduced in support of the Lamarckian hypothesis, but there is nothing approaching a demonstration that somatogenic characters are impressed on the germ through the action of hormones or internal secretions. It is not unlikely that chemical processes of this type are involved in the modification of hereditary characters, but Cunningham supplies no clear proof that such is the case.

With the thought that the serum of organisms with blood or lymph might be a medium whereby external conditions could affect the germplasm, Professor Guyer and his associate, Dr. E. A. Smith, devised a series of experiments to test this possibility, which yielded some positive results of a rather striking character. By injecting intravenously into pregnant rabbits fowl-serum sensitized against the rabbit crystalline lens, several young were produced with one or both eyes seriously defective, while others had eyes which were abnormal. The commonest abnormality was partial or complete opacity of the lens, accompanied by reduction in size. This anomaly was transmitted by breeding to the eighth generation and actually became more pronounced in later generations. It was found possible to transmit the defect through the male, thus proving it to be a genuine hereditary character. The character apparently behaves as a Mendelian recessive. So far as a layman can judge, these experiments were carefully planned and executed, and the results secured quite reliable. They should of course be repeated by other investigators.

The same investigators secured evidence that an animal can build effective antibodies against its own tissue. They succeeded in producing a young rabbit with defective eyes from a mother who had been injected repeatedly, before and after pregnancy, with pulped rabbit lens; and found it comparatively easy to produce spermatotoxins by injecting rabbits, either male or female, with rabbit spermatozoa.

Guyer suggests, on the basis of these and other serological data, that we may have in the serological mechanism a means whereby hereditary characters could be initiated or modified.[1] It should be added that Guyer offers this suggestion only as a working hypothesis, and by no means as an established theory. The feature of the hypothesis which

[1] References for the foregoing sketch of Guyer and Smith's work are: Guyer, M. F., "Immune Sera and Certain Biological Problems," *Am. Nat.*, Vol. LV, 1921, pp. 97–115; "Serological Reactions as a Probable Cause of Variations," *Am. Nat.* Vol. LVI, 1922, pp. 80–96; "Orthogenesis and Serological Phenomena," *Am. Nat.*, Vol. LVI, 1922, pp. 116–133; "Studies on Cytolysins: III. Experiments with Spermatotoxins," *Jour. Exp. Zool.*, Vol. XXXV, 1922, pp. 207–223; Guyer, M. F. and Smith, E. A., "Transmission of Eye-Defects Induced in Rabbits by Means of Lens-Sensitized Fowl-Serum," *Proc. Nat. Acad. Sci.*, Vol. VI, 1920, pp. 134–136.

specially concerns us is its implication that serological reactions may be a mechanism whereby somatogenic characters are impressed on the germ-plasm. Assuming that the results of this series of experiments will be confirmed by other investigators, we may say that the influence of serological reactions on heredity has been demonstrated, even though it is possible to interpret those results on the hypothesis of parallel induction.[1] Guyer's work has evidently brought us nearer an identification of the processes whereby somatogenic characters are transmitted to offspring.

It would be erroneous, however, to suppose that a transmissionist mechanism might be constituted by serological reactions alone. That would be to fall into the fallacy so common among biologists to-day, that, namely, of assuming that vital organization of any sort can be accounted for in terms of physicochemical processes alone. It may be pointed out that the serological data to which Guyer appeals do not include any *morphological* data as to the physiological or phylogenetic changes he discusses. Serological and all other types of chemical reactions occurring in the organism, whether or no they play a part in hereditary modifications, are *organized* in an exceedingly complex fashion, and such organization is a *sine qua non* for the maintenance of the organism, and its successful adaptation, through hereditary modifications, to a changing environment. Hormones and other chemical substances in the body must be of quite specific kinds and amounts, and circulated in very definite ways, if the organism is to survive, and physicochemical action as we are acquainted with it cannot organize or keep itself organized, in this complex and adaptive fashion. Some sort of organizatory agents must be assumed if such organization is to be accounted for or its existence, as such, admitted. That is true of the processes whereby modifications are impressed on the germ-plasm, however those modifications be induced.

Summarizing our discussion of the general problem, we may say that no valid argument against the Lamarckian hypothesis can be drawn from the failure hitherto to identify, or identify completely, a transmissionist mechanism; that the demonstration of the causal rôle played by functional activity in the genesis of hereditary variations establishes *ipso facto* the existence of a transmissionist mechanism; that the mnemonic theory of heredity is a significant descriptive category, but does not represent a contribution toward the identification of a transmissionist mechanism; that there is some direct evidence that serological reactions are involved in the hereditary transmission of

[1] Guyer, M. F., "Immune Sera and Certain Biological Problems," *Am Nat.*, Vol. LV, 1921, p. 110.

modifications somatically induced; but that the orderly, adaptive character of hereditary variations and their transmission to offspring can scarcely be accounted for save on the assumption that organizatory agents are operative therein.

(7) Methods and Assumptions of the Analysis

The methods and basic assumptions of the analysis have received a good deal of incidental discussion in the course of the analysis itself, and we can therefore afford to be brief in the more explicit statement and justification thereof now to be presented.

We have proceeded on the principle that scientific hypotheses must be appraised as to their theoretical validity by all the empirical consequences legitimately deducible therefrom. It is our belief that all anti-Lamarckian hypotheses, when appraised by this criterion, show themselves to be quite untenable. They all lead logically to the supposition that external agents have intervened in the processes of phylogenesis, so far as active specific adaptations to the environment are concerned, whereas the Lamarckian hypothesis, when subordinated to the synthetic theory of variations herein proposed, does not carry any such implication. These consequences of the anti-Lamarckian hypotheses are clearly incompatible with the theory of evolution, and with the fundamental postulates of modern science. In comparing Lamarckian and anti-Lamarckian hypotheses, however, it was not claimed that the factors and processes assumed by the latter hypotheses are not really operative in the genesis of variations, but that no combination of those factors and processes can account for hereditary active adaptations as such.

We do not anticipate any objection to the criterion whereby these several hypotheses have been appraised, although there may be criticisms of the applications made of it. One such criticism has been expressed by H. S. Jennings.[1] He says our hypothesis, that a hereditary active adaptation to a specific feature of the environment is established through functional activity related to that feature of the environment, itself implies the same sort of preëstablished harmony between the organism and the environment, as is shown to be implied by the interpretation of such adaptations on the germinal hypothesis. "Species A," says Jennings, "cannot be brought into coördination with species B even by species B unless the constitution of A is preestablished of such a character that the required alterations can be made."

[1] In a letter to the writer.

Let us examine this criticism. *Any* genetic theory must concede that organisms which are capable of adapting themselves to specific features of the environment are, to that extent, harmonized in advance with the environment, or, in other words, that they are adapted to those features of the environment. But capacity for adaptation to the environment is a very different thing from actual adaptations to specific features of the environment. It is just as different as a capacity for learning the Chinese language is from the actual mastery of that language. Our contention is that hereditary adaptations to specific features of the environment—as the specific adaptations of a parasite to a single host species—are established in much the same way as are adaptations of human beings to the Chinese language or to the science of zoölogy, and that mastery of such language or such science without numerous reactions thereto would be no more miraculous than would adaptations of a parasite to its host without numerous reactions thereto on the part of its ancestors. We may fairly argue that a hypothesis implying that such specific adaptations can be established without reactions to the several objects of the adaptations playing a part in the process is repugnant to any valid criterion for appraising the truth of a scientific hypothesis.

We have not explicitly discussed the question how capacity for adaptation to specific features of the environment, which must be assumed by all hypotheses respecting the origin of adaptations, is to be accounted for. Yet that question is dealt with, by implication, in our discussion of plastic adaptations to language, customs and other features of behavior socially originated, but not inherited in a biological sense. We found grounds for believing that such adaptability is to be interpreted, in part, on Lamarckian principles. We suspect that all kinds of adaptability, however generalized they may be, would necessarily be accounted for, in part, on the same principles. Organisms have always been bound up with the environment and it seems certain that all their various types of adaptability to the environment have originated in and through their interactions with the environment. Interpretation of any type of adaptability, however generalized, on anti-Lamarckian principles would imply much the same logical consequences as the interpretation of any active specific adaptation on the same principles. For any such adaptability is in the nature of a plastic or generalized adaptation, and, therefore, an adaptation referring to a broad group of specific environmental features, but not to all features of the environment. Such an adaptation will therefore not be without its specific reference, and, having such a reference, our analysis will apply to it.

Our application of the criterion stated, namely, that a scientific hypothesis is to be appraised through a consideration of all the empirical consequences implied by the hypothesis, was made necessary by the nature of the problems to be examined. It was shown in our survey of the experimental evidence brought to bear on these problems, that an impasse between Lamarckian and anti-Lamarckian writers thereon had been reached. It was also shown that, although the use of current experimental methods in dealing with these problems might lead to some decisive results, such results could not be other than fragmentary, or provide a foundation for other than an eclectic theory of the origin of variations.

This situation is due in the main to two causes. (1) Physicochemical factors, functional activity, morphological properties, environmental stimuli and other factors or properties to which one or another school of biologists appeals for a causal explanation of phylogenetic changes are *all* necessarily involved in all phylogenetic changes. Experiments cannot therefore be devised in which one or more of these factors are excluded. The results of experiments relative to this problem are therefore susceptible of conflicting interpretations. The most that can be achieved by their means is some more or less probable conclusions as to the causes of hereditary modifications experimentally induced. For such conclusions are arrived at, not by excluding any of those factors or properties from these experiments, but by accentuating the rôle played by the factors or properties whose causal efficacy is to be tested. At best we get dubious and fragmentary results by this method.

(2) The more significant types of hereditary modifications, and particularly active adaptations playing an indispensable rôle in the life of the organism, cannot be produced in the laboratory. There may be some exceptions to this general rule, but with the largest possible allowance for such exceptions there will remain many exceedingly important types of hereditary characters which cannot be studied by experimental methods. Biology must employ other methods of dealing with such characters if it is ever to become a comprehensive science in its own field. This does not mean that observational and experimental data cannot or should not be utilized in the investigation of those characters. Such data most positively should be utilized, and utilized to the fullest extent. They should serve both as material on which nonexperimental methods (supplementary to experimental methods) are employed in seeking a decision on fundamental problems presented by those data, and as evidence whereby to test tentative conclusions arrived at by the use of those methods.

In seeking decisions on the problems presented by the situation thus indicated, we were necessarily limited to a certain procedure. First of all, the situation itself and the investigational difficulties which it presented had to be made clear. In particular it had to be shown that functional activity, physicochemical processes, morphological properties, germ-plasm and soma (in sexually reproducing species) and environmental stimuli are all bound up together and that none of these factors are ever excluded from phylogenetic processes. This we did by means of the *a priori* analysis in our first chapter. The result of that analysis, stated in other words, was the conclusion that the problem of the origin of variations, as at present conceived, rests on unsound assumptions, and that it is, therefore, an artificial problem. That analysis carried the implication that experimental evidence supposed to bear on the problem of variations, thus conceived, actually incorporates that problem within itself, and serves, in the main, only to illustrate it.

Our task was then to show that all those factors or properties, though indissolubly associated together, must nevertheless be taken into account, if phylogenetic changes are to be accounted for. In doing this we took special pains to guard ourselves against the fallacy which we had been criticising. Throughout we regarded structure, functional activity, physicochemical processes, etc., as factors, properties or mere aspects of vital phenomena, which, taken separately, have no independent reality, so far as living organisms are concerned.

In demonstrating the positive rôles, in phylogenesis, of these several factors or properties, we were obliged to make an extraordinary use of inferential processes of reasoning. It is this feature of our procedure which is likely to provoke criticism among biologists and logicians who emphasize the use of experimental methods in the positive sciences. Our justification of the methods employed, or rather of the extraordinary use made of them, has already been indicated, in part: such use of those methods was prescribed by the nature of the problems to be investigated. An additional and, as it seems to us, a definitive justification of the methods employed is that they do not really represent a departure from the well tested methods of experimental biologists themselves. Any science *qua* science is a more or less consistent system of inferences about certain matters of fact. This means that a science is built up not only by the observation and classification of facts, but by working out, through processes of inference, the connections among the facts observed and classified. Positive science begins and ends with observed matters of fact, but in any scientific investigation of complex problems there is a vast system of inferences between

the terminal facts, if we may so speak, at the beginning and the conclusion of the investigation.

In demonstrating (so far as the demonstration has gone) the part played in phylogenesis by the several factors or properties of vital phenomena, we have not departed from the procedure just indicated. The only difference between our use of this procedure and that represented by most discussions of biological problems is that our inferences have been more extensive and systematic. But that, to repeat, was necessitated by the nature of the problems under consideration. We can justly claim that there has been the same scrupulous regard for pertinent matters of fact throughout the inquiry as characterizes the most zealous devotee of experimental biology. We may point to successful applications of the same method by naturalists themselves. The theory of descent was established largely by the aid of inferential reasoning of a complex character; but reasoning which always had reference to facts. The theory was not and, in its broader reference, could not be proved directly. The theory of the localization of genes or germinal factors in the chromosomes was also established by a process of investigation in which systematic inference played a conspicuous rôle. It is necessary to cite additional examples, the list of which could be extended indefinitely. Indeed, inference, on the one hand, and observation and experiment, on the other hand, must be combined in all sorts of proportions, when regarded in a crude quantitative fashion, if the infinitely diverse problems of modern science are to be brought nearer a solution.

Another criticism of our analysis would have a good deal of plausibility. It is easy so to combine factors coming within the general categories distinguished by us as to provide a solution, in general terms, for any problem in phylogenesis that might be presented. For example, in discussing the specific adaptations of flowering plants for facilitating cross-fertilization through the visits of insects, we could readily appeal to organizatory agents for an explanation of the features thereof which could not be reasonably ascribed to functional activity or physicochemical factors. And it was found necessary to lean rather heavily on that category of factors in the interpretation of other adaptations which apparently were not susceptible of an interpretation on the Lamarckian hypothesis, even on such a modified form thereof as we have proposed. The solution of difficult problems may thus appear to be altogether too easy, and the results, therefore, of a highly formal and artificial character.

We confess to sharing this same criticism of our own procedure. And yet we believe it can be defended against this criticism. It will

have been observed that, in dealing with any given problem, the explanatory possibilities of all other types of factors except the organizatory were exhausted, so far as we could do that, before appeal to the latter was made. That category is therefore a residual one to which has been relegated all the processes in the genesis of variations which could not be subsumed under other categories. If that is a correct way of putting the matter, the bias of the analysis, if one there has been, favors the categories of functional activity and physicochemical processes rather than the category of organizatory agents. A justification of this procedure would be that the explanatory possibilities of the more thoroughly investigated factors should be fully exploited, and even magnified, before an appeal is made to factors less thoroughly investigated. It may be, however, that the analysis has a bias which must be corrected by future investigations. While it is certain that further investigation will result in more explicit and specific definitions of organizatory agents, asuming that category will ultimately be adjudged a valid one, than have been indicated by our analysis.

An additional justification of the apparently formal character taken on by certain of our results is supplied by a consideration of the aims of our inquiry. We have attempted only to construct the framework of a valid theory of variations and not to work out such a theory in detail. In doing this it was necessary to consider divers types of hereditary characters and so to design the general theory that they could all be fitted into it. The ground which had to be covered, in this undertaking, is so extensive, and our own field of competency so limited, that we have been obliged to adapt the theory in a formal manner to many of the facts bearing on it. The result could have been more satisfactory, had our command of the relevant data been a firmer one, but still, for the purpose in hand, it may serve passably well. Perhaps the fact that all the diverse types of hereditary characters can be fitted into the theory without excluding or discounting any of them should speak rather eloquently for the validity of the theory.

Our analysis has assumed, without a detailed examination of the matter, that the complex and adaptive structure of the organism cannot be accounted for in terms of physicochemical action alone. Although our grounds for this assumption have been indicated at various points in the discussion, the more systematic examination of the issues raised thereby has been reserved for later chapters. We shall also consider in the course of that discussion the nature of the relationships between functional activity, on the one hand, and physicochemical processes and organizatory agents, on the other.

The basic assumption of the analysis, and the only one for which no

justification is offered, is to the effect that the changes which take place in any system of phenomena are produced by factors within the system itself, and without the intervention of agents or factors external to the system. We have brought all the current hypotheses respecting the origin of variations to the test of this assumption, and have utilized the results of these tests in formulating a theory respecting the origin of variations which shall be compatible with this assumption. The results of this attempt must speak for themselves. In order to account for all types of variations without appealing to causal factors external to the process of variation it has been necessary to assume the existence of factors which are not accepted as such by a large proportion of present-day biologists. We refer to organizatory agents, and to functional activity conceived to play a rôle of a representative character in the genesis of variations. The justification of those assumptions is one of the principle aims of the inquiry as a whole, and no recapitulation of the evidence adduced in their support is here called for. The assumption of a transmissionist mechanism, together with other assumptions not here alluded to, have elsewhere been given such support as seems necessary for the purposes of the inquiry.

We have now examined the various problems which seem to be most crucial for the theory growing out of the critical and constructive analysis presented in previous chapters, and have concluded that none of these problems appears to carry any destructive implications for that theory. But that examination rendered the positive service of indicating directions along which the theory needed qualification. If there have been no fatal errors in our analysis, and if the problems crucial for the resulting theory have been faced and brought into harmony with it, this theory should be capable, in its main outlines, of withstanding the criticisms that may be directed against it.

Our inquiry will now be completed by an examination of mechanistic and vitalistic conceptions of vital phenomena, and by utilizing the results of this investigation in the further development of the theory presented in the preceding pages.

CHAPTER VII

MECHANISTIC CONCEPTIONS OF LIFE, THEIR PHYSICAL, PSYCHOLOGICAL AND LOGICAL ASPECTS

Types of Mechanistic Conceptions

The various theories which interpret life in materialistic, mechanistic or physicochemical [1] terms may be reduced to three general types.

(1) One type of theory maintains that the cell, the tissue, the organ and the organism as a whole are nothing but complexes of physicochemical activities—electrical, atomic, molecular, colloidal, molar—which derive their organization solely from the various material factors entering therein. This theory denies any scientific validity to such categories of (alleged) factors as organizatory agencies, intellectual processes or cultural controls originating in the past, if these be regarded as other or more than the physicochemical activities associated with them in causal series which they apparently enter. Scientific validity is also denied to all distinctively biological conceptions, such as adaptation, selection and functional activity. According to this theory, physicochemical formulae or equations are to be substituted for all such categories and conceptions where genuine scientific treatment is in question. This type of theory is sometimes referred to as the elementalist form of the physicochemical or mechanistic hypothesis.

(2) A second type of theory does not impugn the validity of biological conceptions, such as structure, functional activity, adaptation and the like, or deny every sort of causal significance to intellectual and other mental processes, but maintains that a distinct type of reality is represented by the *complex syntheses* of physicochemical factors into living organisms, and that this includes distinctively vital and mental functions, such as those indicated. While the adherents of this hy-

[1] The terms materialistic and mechanistic are employed in this discussion as synonymous with physicochemical, unless otherwise indicated by the context. These terms are of course often employed to designate concepts which are not physicochemical in the sense intended by the physical and chemical sciences. Materialism, for example, is sometimes taken to mean a particular conception of the physical, and mechanism to denote deterministic or naturalistic conceptions of life, whether or not regarded as physicochemical in character.

pothesis deny the operation of non-material factors in vital phenomena, they hold that there are various higher and lower levels of physicochemical activity, considered in respect to the complexity of organization manifested thereby, and that any given level of organization embraces new qualities or functions which are irreducible to the specific physicochemical factors organized therein, or to qualities or functions peculiar to lower levels of physicochemical activity. These higher qualities or functions, however, have been created by the chemical and energetical factors associated therewith, together with organizational factors peculiar to lower levels of physicochemical activity. Ultimately, therefore, the causal factors in the entire series are of a chemical or physical order. This type of theory is often designated as the organismic form of the mechanistic conception of life.

(3) The third type of theory admits and even emphasizes the vast differences between organic and inorganic phenomena, but attributes these differences to some special form or forms of matter or energy assumed to be operative in the organic, yet matter or energy not fundamentally different in kind from forms of matter or energy in the inorganic. Thus, to cite some representatives of this type of theory, Osborn suggests that vital phenomena now inexplicable by physics and chemistry may be due to an unknown source of energy, or to some "known element which thus far has not betrayed itself in chemical analysis," or to "some unknown chemical element, to which the hypothetical term *bion* might be given," or, finally and more probably, to the "as yet partially explored activities of various chemical messengers in the bodies of plants and animals";[1] while Montague attributes the differences between the organic and inorganic, as manifested in the phenomena of variation, heredity and consciousness, to a special form of energy, variously referred to, in his analysis of specific problems, as protoplasmic stresses, super-forces, superimposed stresses, potential energy, energy, or negative energy,[2] which type of energy is not conceived to be different in kind from energies at work in the inorganic, but as definable in mechanical terms. This third type of theory may be conveniently designated as the *particularistic* form of the mechanistic or physicochemical conception of life.

It is probable, as Professor Pratt has suggested, that many philosophers calling themselves behaviorists, neo-realists, pragmatists or ideal-

[1] Osborn, H. F., *The Origin and Evolution of Life*, p. 6.

[2] Montague, W. P., "Are Mental Processes in Space?" *The Monist*, Vol. XVIII, 1908, pp. 21–29; "Consciousness a Form of Energy," *Essays Philosophical and Psychological in Honor of William James*, pp. 103–134; "Variation, Heredity and Consciousness," *Proceedings of the Aristotelian Society*, New Series, Vol. XXI, 1921, pp. 13–50.

ists hold views which are essentially materialistic [1] in character, though they might not agree to such a characterization thereof. Were we to canvass their views systematically, we should doubtless be able to identify varieties of the physicochemical conception of life other than those already sketched, but we should probably find that, when stripped of epistemological coverings, they approximate one or another of the conceptions here distinguished, or fall somewhere in between these. We cannot in this inquiry examine all possible combinations of mechanistic doctrines, but must content ourselves with an analysis of basic doctrines to which all such combinations might be reducible. This limitation of the analysis is justifiable, from the viewpoint of the inquiry as a whole, for the reason that we shall present in outline an independent analysis of vital phenomena with a view to identifying the basic categories of factors operative therein; and this analysis will carry critical implications for materialistic conceptions of the types characterized, as well as of other types not here distinguished.[2]

Adventitious Advantages of the Physicochemical Conception

It will clear the ground for our analysis of the physicochemical hypothesis in its various forms to point out certain adventitious advantages enjoyed by this hypothesis—advantages which are not balanced by like advantages accruing to a theory allowing for primary factors in living nature other than the physicochemical.

Let us assume for the moment that there are non-material factors in vital phenomena, and that these factors are to be credited with some at least of those characteristics which distinguish organisms from inorganic substances. We can then point out many advantages which those holding to an exclusively physicochemical interpretation of life would enjoy over those claiming that there are also non-physical factors of some sort in life phenomena. While those advantages do not at all touch the merits of the controversy between the two parties, they may have contributed to the popularity and scientific predominance of the physicochemical hypothesis.

In the first place, so soon as current theological and metaphysical views of nature were called in question, and scientific investigation was appealed to for a theory of nature, it was the more accessible aspects of nature which received the major share of attention; and

[1] Pratt, James Bissett, 'The New Materialism," *Jour. Phil.*, Vol. XIX, 1922, p. 339.

[2] It will be necessary, however, to deal more or less explicitly with certain other doctrines bearing on the problems under consideration, as, for example, psychophysical parallelism, behaviorism and radical empiricism.

this direction of attention has persisted until these aspects of nature have come to be regarded as *the* subject-matter of science. By accessibility is meant accessibility to scientific analysis, rather than to the individual consciousness, for the processes of consciousness are perhaps as accessible to the subject himself as are phenomena referred to an external world. But consciousness is, so to speak, *private* to the subject, and not public property which may be investigated in coöperation with others. Conclusions drawn from an investigation of the former are not, for this reason, as susceptible of verification by others, and not so readily erected into laws or principles of universal validity. Besides, the processes of consciousness are more complex and at the same time less stable than are external objects, and definite results are not easily obtained from their investigation, even when the investigator limits himself to his own consciousness. Organic structure or form hardly occupies a more advantageous position in this respect than does consciousness, for it is referred to an external world which acquires its objectivity, so to speak, from matter rather than from form itself, so that when the matter which embodies form is taken away, the form itself vanishes.

Matter, on the other hand, is objectivity itself. It is always open to observation through one or more of the senses. Indeed, matter and energy, according to various current definitions thereof, are constituted by, or concepts of, or entities somehow in, sense-phenomena. Matter and energy may not be the only components or factors of sense phenomena, but manifestations of matter and energy are certainly more accessible to observation and analysis than are manifestations of possible non-material factors in nature.

It might be objected that energy itself is imperceptible to the senses, and hence no more accessible to observation and analysis than vital factors would be, did any such factors exist. We may point out, however, that the existence of a factor imperceptible to the senses was readily inferred from differences in the behavior of the same bodies at different times, as was also the existence of different forms of energy and their transformation into one another. And experiments with a view to determining the properties of energy readily suggested themselves. These terms, "readily inferred" and "readily suggested themselves," are used in a comparative sense only; we mean that the establishment of energy as a primary factor in nature was, under the conditions given, much more easily effected than could have been the establishment of a vital factor, assuming provisionally that there is such.

As a matter of fact, however, a vital factor was postulated to account for the differences between the behavior of organic and inorganic bod-

ies, and it was only after the existence of such a factor was disputed and apparently disproved, by showing that organic bodies conformed to the law of the conservation of energy and that they were susceptible, in various ways, of physical and chemical analysis, that the postulation of a vital factor was abandoned by science. In view of the impressive demonstrations of the continuity of organic with inorganic nature where matter and energy manifestations are concerned, the claims of a hypothetical vital factor could not expect to gain another hearing until the physicochemical hypothesis had been given a detailed application in the interpretation of life phenomena, and the ground thus prepared for determining whether it adequately accounted for these phenomena. The physicochemical hypothesis in a genuinely scientific form got possession of the field first, so to speak, and the burden of proof was thus thrown upon the propounders of the vitalistic hypothesis. The old vitalism was, in fact, overthrown, and it is only in recent years that any new vitalism could really challenge the sufficiency of the physicochemical interpretation.

So far, however, the new vitalism has not been able to detract seriously from the prestige enjoyed in scientific circles by the physicochemical hypothesis. And if we may suppose for the moment that the prestige of this hypothesis is not wholly merited, it is certain, nevertheless, to continue undimmed for a long time to come. The incontestable achievements of the physical and chemical sciences are impressive, and go far to create a favorable attitude toward, even if they do not really substantiate, the claims of those sciences to offer an interpretation of all natural phenomena.

Moreover, certain achievements of those sciences tend to place them in a truly impregnable position as regards their claim to offer an adequate interpretation of vital phenomena. It has apparently been proved that both matter and energy are indestructible, and hence eternal. Moreover, both matter and energy seem to be ubiquitous. Not only do they constitute part if not the whole of all mundane reality, but forms of matter and energy known to us are believed, on good evidence, to be component elements of all the sidereal bodies of which we have any knowledge. Again, physics and chemistry are "exact sciences." Matter and energy are susceptible of the most delicate measurements, whereas sciences dealing with life phenomena are comparatively inexact, except where physicochemical methods are employed. All these attributes of matter and energy lend an impressive, even though it be an illusory, support to a comprehensive physicochemical interpretation of life phenomena.

The physicochemical interpretation of life phenomena profits in a

special manner from the ubiquity of matter and energy.[1] For life is always associated with matter and energy and could not exist apart from them, whereas if there are primary vital factors in nature, they have never been isolated from matter and energy. The most that can be done is to infer their existence. And should we grant their existence, they would apparently not be accessible to a quantitative analysis, and hence it is doubtful if a science thereof could be an exact science. If science be defined as measurement, as is often done, then a genuine science of vital factors, if there be such, seems impossible.

We may add, finally, that materialism represented, historically, an attack on theological and mystical conceptions of nature, and it is even now generally regarded in the scientific world as the only alternative to such conceptions.[2]

For all these reasons, the more extreme pretensions of the physical and chemical sciences, together with the materialistic philosophy commonly combined with them, have acquired a momentum that is well-nigh irresistible. This philosophy is held by the average scientist much as a religious dogma, and defended as such. It is, in fact, *the* religion of modern science, and sanctioned by all those psychological tendencies—suggestion, habit, etc.—which lend support to any generally accredited dogma. So true is this that the belief in materialism is almost exempt, at least in the scientific world, from a critical examination as to its all-embracing validity.

All these are adventitious advantages of the physicochemical hypothesis, and in nowise affect the merits of the controversy between the physicochemicalists and those who maintain the existence of factors in nature other than matter and energy. The accessibility of material factors to scientific analysis creates no real presumption against the existence of factors not so accessible. The ubiquity of matter and energy does not disprove the existence of factors in nature which are not so widely distributed. The indestructibility of matter and the conservation of energy do not disprove the existence of other factors, nor prove that matter and energy have a higher existential status than other factors which may be discovered in nature. The quantitative features of matter and energy, their consequent susceptibility to mathematical analysis, and hence the elevation of physics and chemistry to the rank of exact sciences do not prove that there may not be a genuine science of some other natural factor or element, nor that such a science may not be of as high an order as the sciences of physics and

[1] We do not imply that matter and energy are distributed throughout space, although energy at least appears to be so distributed.

[2] Ritter, W. E., *The Unity of the Organism*, Vol. II, pp. 148 ff.

chemistry. The fact that life is always associated with matter and energy, and that specific organic processes may be partially explained in terms of physics and chemistry, does not disprove the presence and operation in life phenomena of other classes of natural factors. Finally, the general acceptance of the materialistic philosophy in the scientific world creates no presumption for the validity of that philosophy. The history of thought is replete with discredited but once widely accepted philosophies of nature, and the history of biology presents its full share of them. Nor does the claim of materialism to offer the only alternative to supernaturalism lend any support to that doctrine, for this claim rests on a definition of the natural as the material, which begs the question here at issue. All the attributes of matter and energy and all the achievements of the physical and chemical sciences which we have alluded to are ungrudgingly accepted by those who would maintain the existence of factors in nature additional to and other than matter and energy. The issue between them and the physicochemicalists is not touched, therefore, by the character of the subject-matter and the achievements of these sciences, nor by the commanding position which they occupy in the scientific world.

Our purpose in the preceding paragraphs has been to strip the physicochemical hypothesis of its adventitious advantages, of protective coverings, which tend to exempt it from critical examination; and thus set the hypothesis before us naked, as it were, where we can deal with it on its merits. While this may not have been necessary for the more critical student, we cannot but think it an indispensable preliminary for others less critical—perhaps a majority of scientific workers—who may find it difficult to disengage themselves from the spell which the materialistic philosophy has cast over the scientific world for so long.

We have not purported thus far to deal critically with physicochemical hypotheses themselves, as the previous discussion creates no presumption for or against these hypotheses. That is the task to which we now turn. Our procedure in this undertaking will be to employ various distinct methods in the examination of those hypotheses, and compare the results yielded by the application of these several methods. In the present chapter this group of hypotheses are tested by means of a comparative analysis of organisms and inorganic bodies, respectively, with regard to (1) their *physical* components, and (2) their characteristic types of behavior; they are also compared with opposed hypotheses as regards their logical characteristics. In the next chapter a factorial (or epistemological) analysis of the physical and the mental is undertaken. This provides a further test of physichochemical hypotheses, and also an ontological basis for the positive conceptions

developed in connection with the analysis as a whole. In the two chapters beyond that a comparative analysis of physiological and correlated physicochemical processes, including those pertaining to variation and inheritance, is presented. In the next following chapter the morphological, physiological and mental processes of the organism, on the one hand, and the associated physicochemical processes, on the other hand, are compared by the method of concomitant variations, with a view to determining the types and degrees of correlation that subsist between the two series of processes. Then, in a separate chapter, consideration will be given to the more serious objections to the positive conceptions developed in connection with the critical analysis of mechanistic conceptions, and to criticisms of the methods employed by the analysis itself.

Divergent Conceptions of Matter and Energy

The various types of the mechanistic hypothesis which we are to examine hold that the organism and its properties, including its structure and its mental functions, are to be accounted for in terms of matter and energy; or, otherwise stated, in terms of physical objects and events, together with physical systems into which these objects and events may form themselves.

Now, there are widely divergent conceptions of matter and energy, and a systematic evaluation of physicochemical theories of life must take these different conceptions into consideration. Our present purpose will be served for the most part by identifying the data or types of data upon which these conceptions are all based, or for which they attempt to account, although the different interpretations of these and *other* data taken into account by various conceptions must also be considered.

All conceptions of matter and energy are based on the immediate data of sense-perception, and offer an account of sense data, or accept them as *constituting* all that we know of matter [1] and energy. Sensations or sensory qualities are also regarded, in various conceptions of matter, or physical objects, as constituents of minds, or as neutral entities which enter into both minds and physical objects, and

[1] Cf. Hoernlé, R. F. A., *Matter, Life, Mind, and God*, pp. 50–52. In the discussion to follow we shall use matter and physical objects as interchangeable terms, and for the reason that different writers use one or the other of these terms as denoting much the same sort of entities. Essential differences in their conceptions of the matter will of course be noted. Nor will it always be necessary to take account of distinctions between matter or physical objects, on the one hand, and energy or motion, on the other.

so on; but we may for the present ignore the part played by sensations or sensory qualities in theories of things not physical.[1]

Matter and energy have been defined as groups of sense-impressions, together with their sequences, while atoms, molecules, electrons, physical and chemical laws are regarded as concepts whereby sense-impressions and their sequences are compendiously summarized.[2]

According to another conception, sensations or sensory qualities are the phenomenal manifestations of electrons, atoms, electromagnetic vibrations and other "real" entities imperceptible, as such, to the organism; and matter and energy are defined as constituted by these entities.[3] Those holding this conception may or may not regard such entities as more real than the sensory qualities which they are believed to produce.[4]

Another view similar to but differentiated from the one just stated is that sensations or sensory qualities are mental in character, but caused in us by matter which is not only imperceptible, but largely or altogether unknowable. This matter may be the "primary qualities" of objects (often regarded as imperceptible), a thing-in-itself, or some other imperceptible entity or entities.

Still another conception regards sensations or sensory qualities as resultants of interactions between the organism and physical objects external to the organism, together with other conditioning events, such as disturbances in the medium whereby stimuli from the physical object reach the organism.[5]

There is, finally, the conception of radical empiricism, that minds and physical objects are not fundamentally different from each other but are "only differences of empirical relationship among common empirical terms," perceptual experience figuring "in one context as an object or field of objects, in another as a state of mind." The world is said to be made of one primal stuff, pure experience, but there are as many specific stuffs "as there are 'natures' in the things experienced." The physical world, according to this conception, is the stable part of the

[1] Problems indicated by these theories will be considered in the next chapter.

[2] Pearson, Karl, *The Grammar of Science;* Mach, Ernst, *The Analysis of Sensations,* and other works.

[3] This seems to be the prevailing view among the physicists and chemists themselves.

[4] Whitehead, for example, appears to regard sense-objects (sensory qualities), perceptual objects (associations of sense-objects) and "scientific objects" (atoms, electrons, etc.) as equally real. He regards events, however, as of a higher order, existentially, than objects "located" in events. See his *Concept of Nature,* and *The Principles of Natural Knowledge, passim.*

[5] Essentially the conception set forth in Lloyd Morgan's *Emergent Evolution.*

whole experience-chaos; and perceptual experiences are the core of this world, but these are rounded out by conceptual experiences.[1]

Holt has elaborated a special version of the same general conception, according to which mind and physical objects are made of the same stuff, or, in his phrase, the same neutral entities. Colors, sounds, odors, motions, energies, masses and other objects of immediate experience enter into different but neutral manifolds termed minds and physical objects. There are no imperceptible substrata of physical objects as thus constituted. Even logical and mathematical entities are constituents both of physical objects and of minds. But all such neutral entities may exist apart from manifolds designated by these terms. And physical objects are independent of our minds but immediately accessible to them, just as is, for example, the number system.[2]

The ontological status assigned sensations or sensory qualities in these various conceptions has been partially indicated already. They are regarded by the neo-realists as parts or aspects of external objects; by idealists as contents of the mind; by radical empiricists as constituents both of minds and of external objects, depending on the given context or manifold; by critical realists as a product of interaction or interplay between external objects and minds or organisms, and as constituting data, contents or essences whereby external objects can be known or affirmed. Each of these positions is variously formulated by different writers, but these differences need not be considered at present. Nor need we now consider the question which, if any, of these theories furnishes a consistent and adequate account of perceptual and other data subsumed under their several conceptions of the physical world.

The point we are here concerned to make is that any of these conceptions, with its underlying epistemological assumptions, may be combined—how consistently, is not the question—with an exclusively physicochemical conception of the organism. For example, Pearson, a subjective idealist, does not deny that the organism can be accounted for mechanistically, or in terms of concepts and laws which apply to the inorganic, though he holds that our knowledge is not at present sufficent to settle this question![3] Montague, a neo-realist, interprets life and consciousness in terms of special energetical events;[4] Holt, a

[1] James, W., *Essays in Radical Empiricism, passim,* esp. pp. xi, 4, 18, 26–27, 31–34, 80, 139.

[2] *The Concept of Consciousness, passim,* esp. Chaps. VII–IX.

[3] *The Grammar of Science,* p. 344. Hugh Elliot in *Modern Science and Materialism* has expounded a mechanistic conception of life and consciousness based on the same type of epistemology.

[4] *Supra,* p. 181.

radical empiricist (as well as a behaviorist and neo-realist), asserts that "life *is* some sort of chemical process, and nothing further"; [1] Sellars, a critical realist, has formulated in physicochemical terms an organismic conception of life; [2] and so on. These instances will perhaps suffice to show that mechanistic interpretations of life may be combined with any of the main types of epistemological theory, with their various doctrines as to the sorts of entities that constitute the objects and energies dealt with by the physical and chemical sciences.[3]

Now, all these conceptions agree in defining matter and energy in terms of sense-phenomena, or of entities thought to produce these phenomena. But they disagree as to the ontological status of sense-phenomena, and of the imperceptible entities (electrons, atoms, things-in-themselves, etc.) assumed for the purpose of explaining or describing these phenomena. In undertaking a systematic evaluation of the physicochemical hypothesis it seems necessary, therefore, to adopt provisionally some neutral conception of the entities given in or indicated by sense-phenomena, as constituting the objects and energies dealt with by physical science. Perhaps the term *sensory elements* might be an appropriate designation for such a neutral conception, and we shall employ it as such.

The Sensory Elements of Living and Non-Living Bodies

These sensory elements come to us *via* the sense organs and the nervous system, or are inferred from data correlated with these structures. Now, there are very important differences between the totals of sensory elements referred to living and non-living bodies respectively. Non-living bodies embrace only sensory elements correlated with visual, auditory, olfactory, gustatory and cutaneous sense organs—the last named subdivided into organs for warmth, cold, pain and touch proper. The cutaneous sensory elements seem to lie on the borderline, as it were, between living and non-living bodies, though elements correlated with the sensory organs of this group may be referred to non-living bodies.

Living bodies embrace sensory elements of all these groups, *and* elements constituted or indicated by the various organic and motor sensations. Let us specify in some detail the sensations of these

[1] *Op. cit.*, p. 158.

[2] In *Evolutionary Naturalism* and other writings.

[3] Types of idealistic epistemology other than the subjectivistic *may be* exceptions to this statement. Their exponents do not appear to have pronounced definitely on this question. For a suggestive discussion of these types in relation to the general question, see Pratt, J. B., *Matter and Spirit*, pp. 97 ff., 205 ff.

classes which represent sensory elements referred to living bodies, but not referable to non-living bodies.[1]

An important group of organic sensations are correlated with the digestive system. These include (1) hunger, itself including (a) hunger pangs, due to muscular contraction of the stomach, (b) appetite or craving for food, distinguishable from hunger pangs, (c) discomfort due to starvation or depletion of the tissues, and (d) the sensation which accompanies the satisfaction of hunger; (2) thirst; (3) nausea; (4) sensations due to distension of the stomach and other cavities; (5) sensations accompanying later digestive processes in the intestines, bladder, etc.; (6) specific sensations connected with urination and defecation; (7) a sensation localized in the abdomen, and stimulated under conditions attending fright, anger, affection, etc.

Within this general group of organic sensations come also various distinctive sensations correlated with circulation and respiratory processes, such as flushing, heart quavers, etc., as well as sensations involved in states of trepidation, anxiety and panic. Sexual functions give rise to another group of organic sensations, including sexual craving, orgasmic sensation and sexual satisfaction.

Psychologists distinguish, in addition, general sensibility or feeling, including the two opposite qualities of pleasantness and unpleasantness. This general sensibility is characterized, according to varying conditions, as drowsiness, discomfort, fatigue, vigor, weakness and the like. Many pain sensations also belong to the organic group. These include stings, stomach pains, eye pains, earache, headache, toothache, neuralgic pains, muscular soreness, and sensations due to pricks, scratches, sores and burns.

We may suggest, as additions to Warren's list, that specific sensations seem to be correlated with coughing, sneezing and many other physiological processes, both normal and pathological, not mentioned by him. Indeed, work in this field has but fairly set in, and it is certain that many organic sensations not specified in the foregoing list remain to be identified.

Many sensations of the organic group appear not to be correlated with specific receptors or specific stimuli, but such receptors and stimuli will doubtless be identified for a number of these sensations by future investigation.

The motor sensations include those of the kinaesthetic or muscle,

[1] Our account is based on Warren's *Human Psychology*, pp. 201–213, 216–225, from which we borrow much of the phrasing in the following enumeration. This use is by permission of, and special arrangement with, the publishers, Houghton Mifflin Company. The discussion is mainly concerned, of course, with differences between inanimate bodies and the human body.

and the static or equilibrium, senses. Kinaesthetic sensations are correlated with sensory nerves terminating in the striated muscles, tendons and joints, and are stimulated by movements of these parts. It is not known whether sensations localized in muscles, tendons and joints, respectively, are distinct in kind, or only different varieties of one general class of sensations; but there are of course many qualitative differences among the sensations included in this group. Specific receptors for these sensations have not yet been identified.

The static sense gives sensations of position and of motion, which, in combination with kinaesthetic and other sensations, yield information as to bodily postures and movements. The receptor for the static sense is known as the semicircular canals, a structure of the inner ear; its stimulus is alteration in pressure of the endolymph within these canals, due to accelerated motion of the head.

The analysis of differences between groups of sensory elements referable to living and non-living bodies, respectively, could be elaborated indefinitely, but a few additional observations as to these differences must suffice. Not all sorts of visual, auditory and other sensations induced by external stimuli can be referred to human or other animal bodies, though sensations from all these groups can be. Living bodies are not fundamentally different in this regard from inanimate bodies, since none of the latter stimulate all the qualitatively different sensations of these groups. Again, the so-called external, organic and motor senses, correlated respectively with what the physiologists term exteroceptors, interoceptors and proprioceptors, are all senses of the same organism; but this is of course not incompatible with the fact here stressed, that the external senses function mainly in relation to stimuli external or superficial to the organism, whereas the organic and motor senses function only in relation to stimuli from *within* the organism. It may be observed, finally, that, so far as the percipient organism is concerned, sensations of all these classes are somehow bound up together and that these or their derivatives enter into various complex entities, such as percepts, images, memories, feelings, emotions, desires, interests, concepts, etc., which are also referable to the organism. Various sensory elements, if all sensations constitute or indicate such elements, must enter into the same complex entities in somewhat the same ways. We do not imply that these complex entities are constituted of these sensory elements alone.

Now, qualitatively simple sensations or sensory elements are irreducible to terms of one another. They are ultimate data or entities which must be accepted as each after its own kind and no other. And there appear to be no grounds for assigning any class of them to a

higher order, existentially, than any other class. Therefore, the sensory elements which constitute inorganic matter and energy, or purely physical bodies and their motions, cannot constitute or produce the sensory elements referable only to living bodies—those identical or correlated with organic and motor sensations. It is not of course implied that the sensory elements of matter and energy do not condition the sensory elements peculiar to living bodies.

We have here a situation that in many ways is a baffling one. It clearly indicates that a very important group of properties peculiar to the organism cannot be accounted for in terms of matter and energy, as defined in any of the current doctrines thereof. And it suggests just as strongly that matter and energy embrace certain selected groups of sensory elements, those correlated with the exteroceptors, and do not include other groups of sensory elements, those correlated with proprioceptors and interoceptors.

Theoretically, perhaps, the conceptions of matter and energy might be expanded to include sensory elements correlated with these two groups of receptors; practically, however, there seem to be insuperable difficulties in the way of doing this at all satisfactorily, because the physical sciences are apparently limited to the analysis of sensory elements correlated with the exteroceptors. Evidently the sensory elements of the motor and organic groups can be systematically analyzed only by the biological and psychological sciences, though the methods of physics and chemistry are indispensable to such an analysis, just as, for that matter, any *comprehensive* analysis of sensory elements subsumed under the concepts of matter and energy must employ the methods of the psychological and biological as well as of the physical sciences. But the methods of introspective psychology must necessarily play a prominent part in the analysis of sensory elements belonging to the motor and organic groups.

We should still have, however, an arbitrary division of subject-matter between the physical and chemical sciences, on the one hand, and the biological and psychological sciences, on the other hand, since, if all types of sense data are equally significant indications of physical entities, they must all be analyzed in a systematic, unified way if an adequate theory of the physical world is to be worked out. Identification of the ultimate components of the stimuli that excite sensations of particular qualities, and of entities which, as such, do not excite sensations at all—magnetic fields, light waves not registered by the spectrum, other sorts of electromagnetic events, etc.—is capable of throwing a good deal of light on this difficulty. At the same time, the identification of all such entities comes from an analysis of sensations

correlated with the exteroceptors alone, particularly, of course, visual sensations. So that, here, as with the grosser features of the physical world, most important groups of sensations are left out of account altogether. We appear to be definitely excluded from a more detailed analysis of the stimuli to sensations of any sort, except in terms of sense data correlated with only one main group of receptors, when, theoretically, data correlated with the two coördinate groups of receptors are equally significant. It would seem, therefore, that a monistic physicochemical interpretation of living bodies is definitively excluded by the facts of this situation.

Mental and Morphological Peculiarities of the Organism

If the physicochemical hypothesis does not apply to sensations or sensory elements correlated with proprioceptors and interoceptors, neither does it apply, in an exclusive sense, to more complex entities of which those sensations or elements are components. Such complex entities include feelings, conations, emotions, sentiments, volitions, ideals and thought processes. In all of these, organic or motor or both these types of sensations are important—according to one writer, dominating—components, though some at least of these complex entities include other components not thus classifiable, such as external sensations and ideas, so-called.[1]

And we can take these complex entities, as given, and assert that they are associated only with living bodies. There are no adequate grounds for the belief that inorganic bodies have as attributes, feelings, conations, emotions, sentiments, volitions, ideals or thought processes. Radical empiricists have contended that the mental and the physical are but different manifolds of the same stuffs or entities. We are not here disputing that contention, but are comparing the entities, stuffs or properties comprised by two different sorts of manifolds which the radical empiricists would term physical, though, of course, we have found reasons for denying that the organism is only a physical manifold. Holt has contended, it is true, that pains, pleasures and other affectional qualities may exist in the outer world, as in a pleasant landscape, a sad and painful street brawl, and the like; [2] and James, by way of supporting the same general theory, speaks of a hateful man, a mean action, a weary road, a jocund morning, a sullen sky and so on. Holt evidently intends his statements in this connection to be taken quite literally, though James does not, for the latter admits that the "appreci-

[1] Warren, H. C., *Human Psychology, passim,* esp. p. 231.
[2] *The Concept of Consciousness,* pp. 110–111.

ative attributes of things" are inert as regards physical nature except that which "our own skin covers." [1]

In any case, if we say that inanimate objects are sad, weary, joyful, affectionate, as are human bodies, we shall have to invent other terms whereby to designate "affectional facts" referable to our bodies, and hitherto named by those terms. The same considerations apply to imagery, volitions, ideals, thought processes and other complex entities associated with living bodies, Holt to the contrary, notwithstanding. [2] We can hardly allow any claim that a context or manifold which *is* an inanimate body itself has feelings, emotions, sentiments, memories, thoughts or ideas. The absurdity of such a claim would be exposed, should we attempt to impute specific entities of these types to inanimate manifolds. Can we say that a mountain, house or chair is angry or afraid, has visual or auditory images, solves a problem in mathematics or logic, formulates and executes a plan, experiences hunger, sexual craving, the toothache or any of the other sensations peculiar to human or other animal bodies? Can we even say that inanimate objects have visual, auditory or other sensations correlated with the exteroceptors? Or that they perceive other objects by means of these external sensations? A theory that would deny all ontological distinctions between organic and inorganic manifolds cannot answer such questions, except by a flat denial of the empirical differences between the two types.

However, sensations and perceptions correlated with the exteroceptors must here be left in an ambiguous position, since we are conducting our analysis of physicochemical conceptions of life on a neutral basis, as regards the divergent theories of knowledge now current, with their various ontological interpretations of those sensations and perceptions. We shall attempt, in the next chapter, to evaluate these divergent conceptions and formulate, provisionally, a conception of sensation and of the entities indicated thereby which shall transcend the bifurcation of sensory elements revealed in the present chapter, and the fragmentary conceptions of the physical world which this bifurcation has encouraged.

Before passing on to other matters, we must state that the various physicochemical conceptions of the organism purport to account, in their several ways, for the properties so far considered, as referable to living bodies only. As regards these conceptions we have only attempted to show, up to this point, that they cannot account for the sensations or sensory elements of the motor and organic groups. None of them, so far as we know, have attempted to do so, except in

[1] For James, see *Essays in Radical Empiricism,* pp. 141–151.

[2] *The Concept of Consciousness, passim.*

general terms not referring specifically to these groups of entities. But we shall consider later both the implications of these various conceptions as to those entities; and their more explicit accounts of other properties referable only to living bodies, that have been discussed so far.

We may now proceed to another group of properties referable only to living bodies, and closely related to the properties previously considered. These are the morphological and functional properties of the organism, or types of relations found only in the organic. These properties of the organism include all types of relations in the organic which are investigated by the various branches of biology, except perhaps bio-chemistry and bio-physics. The branches of biology dealing with these properties of the organism include taxonomy, anatomy, histology, cytology, embryology, genetics, physiology and psychology, besides sciences dealing with the problems of evolution, such as paleontology, ecology, etc. These sciences are of course subdivided in various ways, as, for example, in relation to plant and animal kingdoms, different groups within the same kingdom, and so on. Even philosophy and the social sciences would be included in a comprehensive enumeration of the intellectual disciplines dealing with the functional and morphological properties of living organisms, for these investigate problems of human life and experience falling outside the scope of other sciences dealing with the organism.

Some of the more significant features of these morphological and functional properties may be alluded to. One of these is the specific character of the chemical compounds and physical processes in organisms of a particular species or group, the specific proportions (within limits) of these various compounds and processes, and the specificity of the spatial and temporal relationships between the various physico-chemical processes of the body. A feature of special interest, in this connection, is the fact that the organism builds up its own chemical compounds and elaborates its various forms of energy from materials taken in from the outside. The orderly processes of mitosis, embryogeny and postnatal development are equally significant features. Regulatory processes of various types, as in regeneration, defensive reactions to toxins, the regulation of bodily heat and the like should also be mentioned in any general characterization of the functional and morphological properties of the organism. Adaptations, both hereditary and acquired, to specific features of the environment constitute another feature of great interest. Here come many types of adaptation to the medium, to specific food substances, to organisms of other species and to sensory stimuli of various sorts, as well as adapta-

tions of constituent structures of the given organism to other structures of the same organism. Taken together, the ensemble of morphological and functional properties thus briefly indicated constitute highly complex, quite specific, yet variable and indefinitely diverse systems of relations which are found only in the organic.

It is quite significant that sensation, perception, imagery, memory, thought, feeling, emotion, desire, volition, are bound up with structures or systems of this sort, which are peculiar to living bodies; and that the environmental factors interrelated with these processes are also, in large part, referable only to bodies characterized by this type of relations.

These various properties are conceded by all schools of biological thought to distinguish the organic from the inorganic. Do they signify the operation of factors in the organic that are fundamentally different from any of the factors in the inorganic? The answers to this question presented by the various types of the mechanistic conception will now be considered.

The discussion in this chapter will be concerned chiefly with the mechanistic interpretations of mental processes, particularly inference, judgment and meaning; while the mechanistic accounts of other functional and morphological properties will be considered only in respect to their logical form, leaving to a later chapter the detailed exposition of the consequences implied by these accounts.

All types of the mechanistic conception purport to account for the morphological and functional properties of the organism, including mental processes of all types, in terms of the sensory elements subsumed under the concepts of matter and energy, or of physical objects and their motions. In logical terms, the mechanistic theory of life is a type of rationalism, which claims, by a reductive analysis, adequately to account for all the distinctive properties of the organism in terms of matter and energy. In form, it is similar to theories which attempt to account for the whole of reality, including organisms, in terms of consciousness or of thought. The logical procedure implied by these and other types of rationalism can be and has been criticized, but since this procedure is defended by those who employ it in deducing their various types of rationalism, and since, quite apart from this procedure, any one of these rationalisms might be true, it would seem more promising, in the examination of mechanism, to center our attention on the empirical differences between organisms and inorganic bodies, with a view to determining whether these differences can be accounted for in terms of factors common to both the organic and inorganic.

Elementalist Versions of the Physicochemical Conception

Let us set forth very briefly typical mechanistic interpretations of various mental processes.[1] Jacques Loeb, the leading protagonist of the mechanistic conception in its elementalist form, maintains that "the term consciousness or soul is applied by metaphysicians to phenomena of associative memory, and that the latter depends upon a physical mechanism which must be just as definite as, for example, the dioptrical apparatus of our eye." [2]

In support of this contention Loeb offers a proof that conscious volition is only a function of this mechanism of associative memory. His argument is that, in volition, an innervation of some kind is "caused directly or indirectly by an external stimulus"; and that this innervation produces (1) a coördinated muscular activity and (2) "the sensations that in former cases accompanied or followed the same innervation," these sensations depending on the activity of associative memory. But "it happens that in such cases the reaction-time for the memory-effect of the innervation [the idea of sensations formerly accompanying the same innervation] is shorter than the time for the muscular effect," and this gives rise to the belief that consciousness (here the centrally aroused sensations) wills the muscular activity, whereas both are caused by the innervating process, a fact which escapes immediate observation. "The will of the metaphysicians is then clearly the outcome of an illusion due to the necessary incompleteness of self-observation." [3] Loeb would presumably give a similar account of other psychic phenomena, for he holds that the activity of associative memory recurs in all such phenomena as the elemental component.[4]

Associative memory is defined as a "mechanism by which a stimulus brings about not only the effects which its nature and the specific structure of the irritable organ call for, but by which it brings about also the effects of other stimuli which formerly acted upon the organism almost or quite simultaneously with the stimulus in ques-

[1] It will be convenient, in the case of several writers who have formulated such interpretations, to present at the same time their several accounts of other functional and morphological properties peculiar to the organism. This will serve us in our attempt subsequently to appraise those accounts.

[2] *Comparative Physiology of the Brain and Comparative Psychology,* p. 251. Quotations from this work authorized by G. P. Putnam's Sons, publishers, New York and London.

[3] *Ibid.,* pp. 215–216.

[4] *Ibid.,* p. 12.

tion."[1] And the mechanism of associative memory, as well as all other life phenomena, is due ultimately to "motions or changes occurring in colloidal substances."[2]

Loeb claims that this view of associative memory is supported by the results of experiments on the higher animals, namely, the extirpation of the cerebral hemispheres, after which associative memory is completely lost and nothing remains that could be interpreted as a phenomenon of consciousness.[3]

A brief comment on this argument may be offered. The evidence offered by Loeb does not establish his interpretation of the case, though it illustrates something else nobody is concerned to deny. The first "proof," that in the alleged conscious volition it is the innervation which causes both the muscular activity and the (conscious) sensations, is only an *assumption* that this is the case, not a demonstration that the centrally aroused sensations do not influence the given muscular activity. For it is recognized that these sensations precede the muscular activity, and the assertion that the reaction-time for the memory-effect is shorter than the time for the muscular effect is only another way of stating the same fact. It is quite possible to hold that these prior sensations are *causal* antecedents of the given muscular effect, and Loeb's argument does not exclude this interpretation of the case.

The argument from the loss of associative memory consequent on extirpation of cerebral hemispheres is equally inconclusive. No one denies that associative memory is correlated with the cerebral hemispheres, but this does not necessarily mean that associative memory is *completely identified* with the cerebral hemispheres. For situations of this sort, where two or more distinct types of factors are, or are alleged to be, involved, the presence or absence of a disputed factor cannot be determined by removing the *entire situation,* as is the case in the experiments to which Loeb appeals. What is termed *analysis in situ* is necessary to settle such questions. Loeb's argument is on a par with the (hypothetical) argument that because free oxygen is necessary to most organisms, that element alone produces those organisms; or that because hereditary human traits are involved in human behavior, these traits are the exclusive cause of human behavior. Evidence of the *type* cited by Loeb could be adduced in support of these statements; only *its* worthless character would be more obvious.

[1] *Ibid.,* p. 12; see also *Forced Movements, Tropisms and Animal Conduct,* pp. 164–165.

[2] *Comparative Physiology of the Brain,* etc., p. 14.

[3] *Ibid.,* p. 236.

Loeb's arguments only illustrate the fact that psychical phenomena are associated with "physical mechanisms," but this was proved a long time ago, and nobody now denies the fact.

H. C. Warren has elaborated, from the same general standpoint, a double-aspect theory of the mind-body relation which is different, he claims, both from parallelism and from interactionism.[1] This relation as he conceives it is similar to the surface-mass relation of matter, which (surface and mass) are perceived by two distinct senses.[2] The two aspects do not, in either case, belong to independent series, but are different manifestations of the same series. They are not causally related, nor are they parallel.[3] As thus conceived, consciousness or its prototype is assumed to pervade the whole organic world.[4]

Now, although in this and other papers Warren says a good deal about the psychical aspect of the mind-body relation, in the end the really efficient factor in the behavior of the organism, according to his conception, is not consciousness but the neural processes correlated with it. "Whatever the rôle of consciousness in nature," he says, "it is *not* the guidance of motor activity. The mechanics of intelligent activity follows the same pattern as other movements and transformations of energy—except that, owing to the intricacy of the nervous system, adaptive actions are vastly more complex than the simple types of change found in inorganic nature. The laws of physics and chemistry hold for intelligent organisms as well as for atoms and electrons." [5] Various psychical phenomena are interpreted in accordance with this view. It is argued, for example, that the 'prophetic insight' of consciousness is amply explained by the mechanism of nervous coordination;[6] and that purposive behavior can be similarly accounted for.[7]

The status finally assigned by Warren to the psychical aspect of the mind-body relation comes out in his statement that, "however much my actions may be determined mechanistically or unconsciously or subconsciously, it is my *conscious* experiences—my perceptions, feelings, imaginings and thoughts—that mean life to me. The proved value of consciousness is the subjective life which it furnishes to the individual." [8]

[1] "The Mental and the Physical," *Psych. Rev.,* Vol. XXI, 1914, p. 83.
[2] *Ibid.,* pp. 81, 83.
[3] *Ibid.,* p. 83.
[4] *Ibid.,* p. 84.
[5] "The Mechanics of Intelligence," *Phil. Rev.,* Vol. XXVI, 1917, p. 619.
[6] "The Mental and the Physical," p. 92.
[7] See "A Study of Purpose," *Jour. of Phil., Psych. and Sci. Meth.,* Vol. XIII, 1916, pp. 5–26, 29–49, esp. pp. 20–21, 46–48.
[8] "The Mechanics of Intelligence," p. 620.

Virtually the same comment applies to Warren's concept of consciousness as to Loeb's. He nowhere presents any *proof* that the mental is without influence on behavior, but only offers copious illustrations of the fact that the nervous system is active in behavior wherein mental processes are also involved. He is more explicit than Loeb in defining an ontological status for consciousness, for he does not leave it a wholly insignificant epiphenomenon, as Loeb does, though as impotent so far as the behavior of the organism is concerned. It should be pointed out, too, that, in effect, Warren's theory is a form of psychophysical parallelism, for he only identifies verbally the two aspects of the mind-body relation, otherwise leaving them entirely independent of one another, as do other forms of parallelism. He does not show *how* they are identified.

Organismic Versions of the Physicochemical Conception

The interpretation of mental processes on the organismic form of the physicochemical conception differs from the interpretation thereof on the elementalist form of that conception, in insisting that those processes cannot be completely accounted for in terms of the discrete chemicals and energies combined in the brain, nervous system and body generally, but that it is the *organization* of these chemicals and energies that gives rise to mental processes. This conception, in its various formulations, is termed the organismic conception, evolutionary naturalism or emergent evolutionism, though perhaps not all who accept one or another of the theories thus designated could be strictly classified as adherents of the physicochemical conception of life.

H. S. Jennings' version of this hypothesis may first be presented. He begins by criticising the physicochemicalists of the elementalist school for attempting to explain the behavior of organisms in terms of the specific chemicals and energies embodied therein, which, he holds, is an impossible task; and accuses them, because of the failures necessarily entailed by their methods, of playing into the hands of the vitalists.[1] While their methods yield brilliant experimental results, they cannot, through these methods, furnish any satisfactory account of the structure and behavior of the organism as a whole. This failure, however, does not justify vitalism and finalism, nor refute mechanism. The trouble is that the physicochemical hypothesis is formulated in such a way (the elementalist form) as to disqualify it for the task expected of it.

[1] "Diverse Ideals and Divergent Conclusions in the Study of Behavior in Lower Organisms," *Am. Jour. Psych.*, Vol. XXI, 1910, pp. 349–370.

A better formulation of the hypothesis (the organismic form) will repair this defect. Jennings' own formulation is this: "What happens in any system of matter and energy at any period is determined by the configuration of matter and energy at a preceding period. Experimentally, therefore, differences in resulting conditions in any two cases will always be found preceded by differences in foregoing conditions, so that nothing happens without its determining factors in the previous configuration of matter and energy. If we search with sufficient care, we shall always be able to find in matter and energy a determining factor for everything that occurs. Two identical combinations of matter and energy cannot produce different results, nor two different combinations absolutely identical results." [1]

This does not imply, says Jennings, that there may not be, in addition, subjective properties, nor that it is improper to make other explanations than the mechanistic one, which take such properties into consideration.[2] But this concession to opposing hypotheses is little more than a verbal one, as it is argued throughout that events in any system of matter and energy are always determined exclusively by the configuration of matter and energy preceding such events. This does not mean, however, that there are not in living things configurations of matter and energy not found elsewhere, nor that methods of action are not manifested thereby which are not found elsewhere.[3]

Jennings stresses throughout his discussion of this problem, the importance of the rôle played by the arrangement of the chemicals and energies found in living organisms. This, however, does not set the organism apart from inorganic arrangements of similar materials, such as a clock, doorbell, steel-trap, or musical instrument. Both in inorganic and organic arrangements the same substance may react to a given reagent in different ways at different times; and a mass of substance may, in either type of arrangement, respond in the same way to different reagents.[4] It is suggested on the basis of these considerations, that "the physicist and chemist must study organisms in order to fully understand physics and chemistry, just as the biologist must study physics and chemistry in order to understand organisms." [5]

The building up of the arrangements of living matter must be accounted for in terms of matter and energy, and of preëxisting configurations thereof. Little has been done in tracing out these evolu-

[1] *Ibid.*, p. 359. The original is in italics.

[2] *Loc. cit.;* and "Life and Matter from the Standpoint of Radically Experimental Analysis," *Johns Hopkins University Circular,* No. 10, 1914, pp. 19–20.

[3] "Diverse Ideals and Divergent Conclusions, etc.," p. 359.

[4] *Ibid.*, pp. 361, 362.

[5] *Ibid.*, p. 364.

tionary processes, but some hope of progress is offered by the study of the rearrangement of particles in the hysteresis of colloids, while more has been accomplished in tracing the "external manifestations of these changes in the individual organism." [1]

In a later paper,[2] Jennings argues for substantially the same hypothesis, there termed experimental determinism. The main contention of this paper is that the perceptual determiners of events, those discoverable experimentally, are adequate to account for those events. It is conceded, however, that non-perceptual agents may be clothed by the perceptual determiners, but this would not violate the principle of experimental determinism. The latter doctrine, even when thus qualified, is rejected by vitalists like Driesch, although the latter is not opposed to "absolute determinism." Jennings does not claim that conscious states are "nothing but" configuration and motion, but they do result from configurations and motions. There may, however, be an indefinite number of diverse conscious states, each with its specific configuration and motion.[3] The casual significance of conscious states is here, as in the earlier paper, left somewhat in doubt, but this may be deduced from the main contention of the hypothesis, that there must always be "a systematic correspondence of later *perceptual* diversities with preceding ones."[4]

In a more recent article,[5] these views are reaffirmed and applied in an interpretation of human conduct. States of consciousness are left with the same dubious status, but are apparently made derivatives of perceptual determiners, or configurations of matter and energy. There is an apparent movement on the part of Jennings' thought, considering these papers in their chronological order, away from traditional conceptions of the mental and the physical, toward the type of descriptive analysis advocated by Mach, Pearson and others. But this perhaps does not imply so much a qualification of the hypothesis in the form originally given it, as a greater preoccupation with methods of analysis in the domain of experimental biology. Jennings always holds to the doctrine that "perceptual diversities" are to be accounted for *primarily* in terms of themselves, and not in terms of conscious states or other non-perceptual agents.

Ritter's formulation of the same general conception is summarized in his statement that "all the manifestations which in the aggregate

[1] *Ibid.*, pp. 364, 365.
[2] "Mechanism and Vitalism," *Phil Rev.*, Vol. XXVII, 1918, pp. 577–596.
[3] *Ibid.*, pp. 595, 596.
[4] *Ibid.*, p. 592. Italics not in original.
[5] "Experimental Determinism and Human Conduct," *Jour. Phil.*, Vol. XVI, 1919, pp. 180–183.

we call Life, from those presented by the simplest plants to those of a consciously psychical nature presented by man and numerous other animals, result from the chemical reaction between the organism and the respiratory gases they take, oxygen being almost certainly the effective gas for nearly all animals. An essential implication of this proposition is that every living individual organism has the value, chemically speaking, of an elementary chemical substance." [1] In other passages Ritter emphasizes "the fundamentality of objective as contrasted with subjective personality"; [2] proposes "to look upon oxygen as one chemically elementary substance and the organism as another, the reaction between which is basal in the production of consciousness and all life phenomena"; [3] and so on. Throughout the work cited Ritter emphasizes the primary significance of the organism as a whole, and protests against the elementalist hypothesis that it can be accounted for in terms of "ultimate particles," with their groupings and displacements.[4]

Sellars' version of the same general hypothesis goes more deeply into the mind-body problem, and lays special stress on the creative syntheses said to characterize the processes of evolution. There is an evolution from lower levels of organization to higher, as from the physicochemical to the organic, or from the lower levels of the organic to the level of conscious behavior; and higher levels cannot be traced back to the lower without a remainder. Mind is, nevertheless, a physical category, and does not signify the emergence of a new entity, but only a more complex organization of the physical system of which it is an ingredient. Mental processes are simply brain processes, and, being such, are not alien to the physical. Mind is "physical in the sense that it is an internal character of the functioning brain, though it is not a complete physical thing to be known externally by the sense data it arouses."

In formulating this doctrine, here but briefly indicated, Sellars criticises both parallelism and interactionism, and characterizes his conception as a critical development of the two-aspect theory. He rejects both mechanism and vitalism (the former in its elementalist form), but holds that life is a chemical process in the sense that no strange, nonphysical factor enters into it. It is the *form* of the organic that differentiates it from the inorganic.[5]

[1] *The Unity of the Organism,* Vol. II, p. 286. The original is in italics.
[2] *Ibid.,* p. 327.
[3] *Ibid.,* p. 340.
[4] See, for example, Vol. II, pp. 151 ff.
[5] Sellars, R. W., *Evolutionary Naturalism,* esp. Chaps. XIV and XV; *Critical Realism,* Chap. IX; "An Approach to the Mind-Body Problem," *Phil. Rev.,* Vol.

Quite similar to Sellars' evolutionary naturalism, in most respects, is the theory of emergent evolution elaborated by Lloyd Morgan.[1] According to this theory, new and more complex kinds of relatedness emerge in the course of evolution, and these are genuine determiners of events in the several natural systems wherein they have emerged. These natural systems, in order of evolutionary complexity, are the physicochemical, the vital, and the psychical, each characterized by its own peculiar type of relatedness. Events of the higher systems, however, involve events of the lower systems; and events of the lower systems, when involved in the higher, depend on the kind or kinds of relatedness in the latter. But these natural systems do not constitute disparate orders of being, for the higher systems are not characterized by an entelechy, *élan* or any other special agency or activity, but only by types of relatedness not found in the lower systems.

Mind is a universal correlate of matter in all these systems. It is also an emergent quality next above life, just as life itself emerged, in order of evolution, from the physicochemical system. God, Activity, or Mind in a third sense, is immanent in, and the source of, all evolutionary change. While mind as universal correlate of matter does not emerge, there are emergent levels of such mind, as correlate. But only at the level of mind (as an emergent quality) is there positive evidence of some correlation between mind and matter. Mind in this sense of universal correlate does not evolve from matter, but with matter.

This theory, here so briefly sketched, is presented as the antithesis of animism, or any view that affirms the intersection of two disparate orders of being. This of course is Spinoza's doctrine interpreted in terms of modern evolutionism, as Morgan himself fully recognizes.

We cannot here go into the problems, so long debated, which are raised by this as by other forms of psychophysical parallelism. We may note, however, that Morgan's general conception does not seem consistent with his view, not before alluded to, that the behavior of the organism, from the stage of conditioned response onward, is under some sort of conscious guidance—, in his own words, that "the mind is captain in the conning-tower of the bodily ship." [2]

Nor is Morgan's general conception compatible with his theory of knowledge—a version of critical realism—, according to which advenient influence from the physical object produces, by some sort of interaction with the organism, sensory qualities which are "projiciently" referred to the physical object, but not possessed by the latter

XXVII, 1918, pp. 150–163; *The Essentials of Philosophy,* Chap. XXII; and "Is Consciousness Physical?" *Jour. of Phil.,* Vol. XIX, 1922, pp. 690–694.

[1] *Emergent Evolution.*

[2] *Op. cit.,* pp. 60–61, 50.

in its own right; thus combining "in one synthesis the joint outcome of advenient *physical* influence from the thing, and projicient *psychical* reference to the object." [1]

Despite these interactionist tendencies, Morgan's general theory is a type of monism, which conceives the physical and the psychical as correlative, but not intersective, attributes in one order of being. As a version of psychophysical parallelism, it *implies* a physicochemical conception of the organism and its behavior; and *because,* like the other versions of parallelism, it accepts as the psychophysical's fundamental modes of behavior the types of processes investigated by the physical and chemical sciences. The higher emergent qualities, on Morgan's doctrine, are only new sorts of relatedness—relatedness of qualities on the physicochemical level, or of qualities on a level above that, but which are themselves ultimately derived from the physicochemical level.

The last formulation of the organismic conception to be cited here is Alexander's version of emergent evolutionism.[2] This is so similar to Morgan's version of the same general conception, that a brief reference to it will suffice. Alexander differs from Morgan in rejecting the doctrine of psychophysical parallelism, and accepting in lieu thereof an identity doctrine of mind and body; [3] and in maintaining a more explicitly physicochemical interpretation of vital and mental processes, but, as in Lloyd Morgan's version, with emphasis on the distinctive qualities of the vital and mental, conceived as emergents from lower levels of physicochemical activity. These distinctive qualities, according to Alexander, can be exhibited without a remainder in physicochemical terms.[4] His general conception is succinctly formulated in these statements: "The emergence of a new quality from any level of existence means that at that level there comes into being a certain constellation or collocation of the motions belonging to that level, and possessing the quality appropriate to it, and this collocation possesses a new quality distinctive of the higher complex. The quality and the constellation to which it belongs are at once new and expressible without residue in terms of the processes proper to the level from which they emerge. . . ." [5] Morgan and Alexander also differ in their conceptions of knowledge and in other matters related by them to their doctrine of emergent evolution, but these differences are not specially significant for our purposes and need not be presented here.

[1] *Ibid.,* pp. 41–53; quotation from p. 52; italics not in original.

[2] Alexander, S., *Space, Time and Deity,* Vol. II., pp. 3–73.

[3] *Op. cit.,* Vol. II, pp. 9–12.

[4] *Ibid.,* II, p. 62.

[5] II, p. 45. Quotation authorized by the publishers, Macmillan and Company, London.

We have not of course done justice to these various versions of the physicochemical conception in its organismic form. We have only attempted to present a bare outline of this conception as thus variously formulated, and have scarcely touched upon the arguments adduced in its support. We are not examining these arguments because, in our judgment, none of them represent an adequate analysis of the crucial differences between the characteristics of living organisms and inorganic objects respectively; and because we hope by a fresh examination of these differences to arrive at more definitive conclusions respecting the validity of the organismic and other forms of the physicochemical conception.

Some of the more common arguments adduced in support of the organismic conception, however, may be briefly alluded to. These will be familiar to students who are at all conversant with the literature of this subject. As we should expect, much is made of the argument that the assumption of efficient non-material factors in the organic would be incompatible with the conservation of energy and other mechanical laws, conceived to be operative in all domains of nature. Much weight is also given to the fact that life and mind are always associated with physicochemical processes, or at least that their existence apart from such processes has not been demonstrated. The argument is then stressed that the behavior of bodies endowed with life and mind can be adequately interpreted in terms of the materials and energies combined in these bodies; and that vital and mental processes themselves are properly regarded as correlative aspects, new qualities, or new types of relatedness of these materials and energies. It is asserted, by way of combating opposed hypotheses, that we have no *direct* evidence of an entelechy, psychoid, *élan vital* or other non-material factors in the organic; the evidence for such factors adduced by Driesch and others being usually passed over somewhat lightly in this connection. Sometimes, as in Jennings' writings on the subject, a methodological postulate is seemingly erected into an assumption as to the nature of the primary factors in vital phenomena. Jennings, it will be recalled, contends that the sequences of perceptual diversities must be explained or described in terms of themselves alone. Methodological considerations are emphasized by many other advocates of the organismic conception. They argue that the assumption of non-material factors in living nature would confuse the biological sciences, because, according to their belief, such factors would not be susceptible of scientific investigation. Other arguments advanced by this school for their physicochemical interpretations of vital and mental processes, when these are regarded as something more than phenomena of, or

series paralleling, the concomitant physicochemical processes, take the logical form of a definition of the physicochemical to *include* the vital and mental, this definition *transcending* the limits within which the physical and chemical sciences themselves are restricted.

These arguments are not examined here, but will be considered, so far as they seem capable of definite appraisal, at various points of our inquiry. What we regard as decisive evidence on the questions involved is presented systematically in this and the following chapters. We shall only observe at this point that, in our judgment, the redefinition of the physical to include the vital and the mental, which is the general form of the *positive* arguments adduced in support of the organismic conception, is not a legitimate one; and that the evidence cited in support of this new definition, such as the operation of mechanical laws in the organic and, more generally, the correlation of the vital and mental with the physical, does not justify that definition. This contention we shall attempt to make good in the ensuing discussion.

Particularistic Versions of the Physicochemical Conception

Our detailed exposition of the mechanistic hypothesis in its particularistic form [1] will be limited to the version thereof, at once the most cogent and the most popular of its many versions, which interprets psychical and other distinctive attributes of living organisms in terms of a special type of energy alleged to be operative therein. This general conception, or suggestions of it, have been advanced by Leibniz, Lotze, Ostwald, Bechterew, Pearson, Benjamin Moore, McDougall, Schneider and many other thinkers interested in questions of this order. In a vague form, indeed, it was the central conception of the older vitalism, and, during the ascendancy of the latter, dominated biological science generally, on its theoretical side. This general conception has been revived recently, in a new form, as a means of repairing the crudities from which the more orthodox mechanistic conceptions are conceded to suffer. Together with organismic versions of the physicochemical conception, and their various two-aspect or parallelistic doctrines of the psychophysical, it constitutes a reforming movement within the mechanistic school of thought on biological and psychological questions, designed, as it were by a sort of compromise, to meet the vitalistic challenge of the mechanistic position, and rescue the latter from the eclipse which seems to threaten it.

[1] See p. 181, for different versions of this form of the hypothesis.

The most cogent of these newer energistic conceptions is that formulated in a series of papers by Professor W. P. Montague, and we shall limit our more detailed exposition of the general doctrine to Professor Montague's version thereof.[1]

Montague prepares the way for an identification of consciousness with special forms of energy, by launching a polemic against the Cartesian antithesis of mind and matter, arguing that mental processes exist in space, and that material and mental processes, respectively, have as characteristics both quantity and quality. He thereupon identifies various resemblances between consciousness and potential energy, and on the basis of the analogy thus afforded proposes to identify consciousness with potential energy. In his latest paper variation and heredity are also interpreted in terms of potential energy.[2]

His conception of energy is made to fit the vital and mental processes to be accounted for. This energy is variously designated protoplasmic stresses, super-forces, higher space derivatives, intensive energies, negative energy, anergy, or simply potential energy. The general conception is that of potential energy defined as stress or force, with various intensive phases thereof. These intensive phases of potential energy are analogized with the variable velocities, accelerations and changes of acceleration characterizing kinetic energies. The specific functions of consciousness, and the various processes of variation, heredity and development are interpreted in terms of potential energy thus characterized. A doctrine termed *hylopsychism* is advanced as a general theoretical foundation for this group of hypotheses. According to this conception, all matter is instinct with something of the cognitive function,[3] and every material event carries with it self-transcending implications of other events, past, future and contemporaneous with itself. Consciousness is a special and higher type of such self-transcending implications, namely, the type involved in brain processes.[4] This superior implicativeness of brain processes makes possible an intelligent, purposive adjustment to an environment extending

[1] This series of papers, in order of publication, are: "Are Mental Processes in Space?" *Monist,* Vol. XVIII, 1908, pp. 21–29; "Consciousness a Form of Energy," *Essays Philosophical and Psychological in Honor of William James,* by his Colleagues at Columbia University, 1908, pp. 103–134; "A Realistic Theory of Truth and Error," *The New Realism,* by Holt, Marvin, Montague, Perry, Pitkin and Spaulding, 1912, pp. 252–300; "Variation, Heredity and Consciousness," *Proceedings of the Aristotelian Society,* N. S., Vol. XXI, 1921, pp. 13–50. These papers will be referred to below by the respective abbreviations, M.P.S., C.F.E., R.T.T.E., and V.H.C.

[2] V.H.C., pp. 17–36.

[3] R.T.T.E., p. 283.

[4] *Ibid.,* p. 283.

immeasurably beyond that constituted by mere mechanical and chemical contacts.[1]

The more significant details of this conception may now be added. We shall pass by with but brief notice the less crucial of the arguments advanced in its support.

First of all, the enormous differences between physical and vital (including psychical) systems are fully recognized; and the causal significance of vital organization is conceded and even emphasized. In one of his earlier papers, Montague specifies certain cardinal differences between the psychical and the physical. These, considered from the standpoint of psychical systems, may be reduced to the capacities of such systems (1) for forms or universals; (2) for the non-actual things of the past and future; and (3) for self-directed and teleological causality. These three capacities of a psychical system are different expressions of its "essential unity and indivisibility as based upon the primacy of the structure or form of the system considered collectively as a whole over the plurality of its elements considered distributively as an aggregate." [2] Psychical and physical systems are sharply contrasted in that the unity of a psychical system is primary, while the plurality of its elements is secondary, whereas the plurality of elements is primary in a physical system, and its unity only secondary.[3] In a later paper *telogenesis,* or the capacity to initiate novel and pertinent ideals, is distinguished from teleology, or the capacity to actualize the ideals yielded by telogenesis.[4]

Three of the four capacities thus attributed to minds are common to all vital systems, including the germ-plasm with its processes of variation and heredity. These three capacities, defined in relation to vital systems generally, are: "(1) *Telogenesis,* or the capacity to initiate useful or purposive variations; (2) *Teleology,* or the capacity to reproduce or actualize a form or pattern by transmitting it through a kind of induction from one body to another; (3) *Extension in time,* or the capacity of living matter to sum up at each present moment of its existence a long series of past events in the form of an invisible hierarchy of intensive magnitudes." [5]

We shall not raise any question as to the validity of these characterizations and may therefore pass on to Montague's arguments for his interpretation of the characteristics specified, in terms of potential energy. We shall also pass by, with but brief comment, his polemic

[1] *Ibid.,* p. 285.
[2] C.F.E., p. 117.
[3] *Ibid.,* pp. 110–112.
[4] V.H.C., pp. 20–21, 37.
[5] *Ibid.,* p. 37.

against the Cartesian antithesis of mind and matter, designed to facilitate his identification of vital systems with potential energy. He presents a well grounded argument for the proposition that certain mental processes at least, such as pains, odors and sounds, are in space. A legitimate implication of this proposition, granting its correctness, would be that all mental processes have a locus in space or at least a spatial reference. The argument against the spatiality of consciousness, based on the invisibility of the latter, is shown by him to be inconclusive, since, as he points out, potential energy, conceded to be in space, is also invisible.[1] We might add that most if not all forms of energy are, as such, imperceptible to the senses, but are detected by an analysis of the movements and interactions of perceptible bodies distinct, in some sense, from forms of energy associated therewith. We could also say, on general grounds, that if mental processes influence events in space, they could hardly be altogether out of space. These comments will indicate that Montague has in our judgment sufficiently mitigated the Cartesian antithesis of mind and matter to permit a physical interpretation of vital systems, provided such an interpretation is not excluded on other grounds.

The next step in the development of his hypothesis is the citation of certain analogies between consciousness and potential energy. One of these is the invisibility of "energy in its intensive phase" and of the "non-physical elements of consciousness."[2] Another is pervasion, by both, of the space of things which they influence.[3] A third is the subordination of the component elements in the two types of entities to the structures or unities by which they are respectively characterized. The magnetic field illustrates this unity or structure in the case of potential energy.[4] A fourth is the similarity of the respective conditions under which a sensation follows a stimulus, and kinetic energy is transformed into potential energy. These conditions are the obstructions, conflicts and readjustments attendant on the passage of stimulus and kinetic energy into sensation and potential energy, respectively.[5] Montague draws other analogies between the field of consciousness, and the forms of potential energy assumed by him to explain consciousness mechanistically;[6] but these analogies illustrate rather than support (so far as analogy may support) his hypothesis, and need not be recounted here.

[1] See M.P.S., pp. 24–25, and C.F.E., pp. 118–121.
[2] C.F.E., p. 126.
[3] *Ibid.*, pp. 126–127.
[4] *Ibid.*, pp. 127–128.
[5] *Ibid.*, p. 128; M.P.S., p. 28; V.H.C., pp. 39–40.
[6] See V.H.C., p. 39; M.P.S., pp. 23–24; and C.F.E., p. 133.

It is on the strength of these four analogies that Montague proposes to identify consciousness with potential energy of various intensive phases.[1] This general hypothesis was later extended to variation and heredity, with specializations rendering it applicable to the various sorts of phenomena coming within these several "vital systems."

Very ingenious is the elaboration given the hypothesis in these applications, and ingenious, too, is the rebuttal of anticipated objections to it. It would be interesting to consider the details of this analysis, but we must limit our discussion to applications thereof, later specified, which, as we think, can be turned into decisive tests of it. We would say regarding the argument as a whole, that it gives added importance to the energistic conception of vital systems; and renders this conception much more plausible than any other version thereof with which we are acquainted. Professor Montague would doubtless not be disposed to claim that the evidence adduced by him serves to *demonstrate* the correctness of this hypothesis. Analogies of the sort cited are good for suggesting hypotheses, but of course not for testing them. And Montague's various orders of super-forces, intensive magnitudes of potential energy, degrees of anergy or negative energy, etc., are obviously *assumed* to account for the distinctive characteristics of vital systems, since there is no empirical evidence that *such* types of energy or anergy exist. It is perhaps significant that Montague's attempt here is to interpret the known in terms of the unknown (saving the demonstrated forms of potential energy), and that the details of this hypothetical unknown are determined, logically, by the details of the known which it is to account for. It is fair to say, therefore, that the peculiar characteristics of vital systems discussed by Montague do more to render intelligible his hypothetical forms of potential energy than do these latter in elucidating the characteristics of psychical and other vital systems. But these criticisms do not of course disprove the existence of these hypothetical energies, nor their performance of that which is ascribed to them by the hypothesis.

Three crucial questions may be raised as to this hypothesis, which admit of definitive answers, and answers that, as we think, will conclusively refute the hypothesis.

(1) Can the type of implicativeness associated with consciousness, and making possible purposive adjustments to an environment widely extended in time and space, be wholly identified with brain processes, due, on this hypothesis, to various intensive phases of potential energy?

(2) Are the types of energy transformations which occur in the

[1] C.F.E., pp. 129 ff.

brain, and the interchanges of energy between the organism, especially its brain, and the environment, compatible with an energistic interpretation of memories and other *enduring* components of vital systems?

(3) Are the types of processes which occur in vital systems, including the brain and nervous system, commensurable with any types of processes demonstrably due, without remainder, to energy and its transformations?

These three questions when properly generalized are pertinent to all forms of the physicochemical hypothesis, and it will be convenient to examine them in relation to the main types of this hypothesis, concurrently, but of course with the necessary reference to the distinctive features of these several types. The second and third questions will be examined in later chapters, and need not be further considered at this point. The first question, besides other pertinent matters of a logical order, will be examined in the present chapter. It remains to prepare the way for an evaluation of the particularistic answer (Montague's version thereof) to this first question.

Montague's proposed solution of the psychophysical problem generally is thus stated by him: "What I, from within, would call my sensations are neither more nor less than what you, from without, would describe as the forms of potential energy to which the kinetic energies of neural stimuli would necessarily give rise in passing through my brain." [1] This of course is a parallellistic conception of psychophysical relationships, but it does not encounter the difficulties which embarrass other forms of parallelism, since, as before indicated, Montague adapts the forms and behaviors of his potential energy to the processes of psychical and other vital systems interpreted in terms of this energy.

We do not think it altogether fair to criticise this hypothesis in the way Professor Pratt does,[2] who complains that Montague "has done nothing to make the identification of consciousness with brain energy any easier. . . . Once and for all (continues Pratt), by our psychic states we mean one thing, and by the physical states of our brains we mean another; and it makes no difference whether these latter be interpreted as motion or as rest, as quantitative or qualitative, as kinetic or potential, as energy or anergy." [3] While this criticism may perhaps be justly applied to Montague's argument that the empirical differences between matter and mind are less or other than we have been led to believe under the influence of Cartesian dualism, *and* to Montague's

[1] C.F.E., p. 129. The original is in italics. Cf. also M.P.S., p. 27.
[2] "The New Materialism," *Jour. of Phil.*, Vol. XIX, 1922, pp. 344–345.
[3] *Ibid.*, pp. 344–345.

undoubted tendency to proceed by *this* argument to an identification of mind with brain processes, it overlooks the parallelistic features of Montague's hypothesis, wherein the empirical distinctions between the outer and inner aspects of brain-mind processes are fully recognized. Any theory whatever that takes all the empirical facts, with distinctions therein, into consideration must admit some sort of correlatedness between two distinguishable groups of "subsistents," whether this be conceived as in the nature of contrasted contexts, independent series, different aspects, or ontologically distinct groups of interacting entities, as denoted by the mind and the brain respectively. Montague's hypothesis allows for such empirical distinctions between brain states and the correlative psychic states, although attempting to diminish the significance of these distinctions.

Getting back to Montague's own account of his hypothesis, he proceeds, after the formal statement thereof reproduced above, and dealing, on the psychical side, only with sensation, to interpret the "tertiary contents of consciousness," such as love, envy, fear and hate, as higher derivatives of potential energy.[1]

These interpretations are developed with characteristic thoroughness, and it must be admitted that in the course of this elaboration Montague strives valiantly toward an identification of consciousness and potential energy by minimizing the empirical differences between the two. For example, he speaks of interpreting "in physical terms the very sensations themselves"; [2] of the kinetic energy of the stimulus being changed into the potential energy of the sensation; of undifferentiated stress as the general "substance" of sensations; [3] of the infinity-phase of slowness (when the kinetic energy of the stimulus passes into a potential form) as the common stuff of all sensations; [4] of the enduring traces of the potential energies in sensation as memory images; [5] and so on.

Into these particular elaborations of the hypothesis we cannot go further, but will turn to those applications of the hypothesis which seem to offer a crucial test of it. These follow naturally from its application to sensation and memory images. The brain, according to Montague, takes its energy "neat," instead of assimilating it, as do other parts of the body, from energy joined with matter, in the form of food particles. The brain gets its energy in the form of sensory impulses; and "by storing up traces of the specific forms of vibration

[1] C.F.E., p. 132.
[2] V.H.C., p. 40.
[3] C.F.E., p. 131.
[4] V.H.C.. p, 42.
[5] *Ibid.*, p. 45.

that proceed from extra-organic objects and traverse the afferent nerves, it builds up a 'psychic organism,' a life within a life, composed not of differentiated forms of matter, but of differentiated forms of energy. These cerebral energy-forms are the instruments of memory, and they implicate or reveal a world outside themselves both in time and space." By the self-transcending implications of these "cerebral traces" we are enabled to act teleologically, that is, with reference to events of the past and future.[1] These self-transcending implications of brain processes "constitute our consciousness of the spatio-temporal world in which we live." [2]

This hypothesis of consciousness as the self-transcending implications of brain events is integrated by Montague in a broader doctrine termed hylopsychism, according to which "all matter is instinct with something of the cognitive function," and every objective event carries with it self-transcending implications of other events.[3] Consciousness in a broader sense includes all such self-transcending implications.[4] By means of this conception the dispute between positivists and substantists respecting causality may be mediated. This proffered mediation issues in a definition of "cause-effect potentiality, which from the objective point of view can only be defined indirectly as a possibility of other events," as "in itself and actually the consciousness of those other events." [5] In turn, consciousness in the broader sense just indicated is formally defined as "the potential or implicative presence of a thing at a space or time in which that thing is not actually present"; or, more briefly, the actuality of the psychical is the potentiality of the phyiscal.[6]

The crucial question in all this is whether the self-transcending implications of objective events, said to constitute both consciousness and causality, and on the basis of which we act with reference to events remote in space and time, can be identified with potential or other energies of the brain and nervous system. Are such actions and all the types of causality involved therein of a kind that characterize the movements expressing energy in one or more of its several forms?

As before stated, this question can be so generalized as to supply a test for all forms of the physicochemical hypothesis. In this generalized form it would run somewhat as follows: Can action with reference to events remote in space or time, together with all the types of

[1] V.H.C., p. 39.
[2] R.T.T.E., p. 285.
[3] R.T.T.E., p. 283.
[4] *Ibid.*, pp. 283–4.
[5] *Ibid.*, p. 279.
[6] *Ibid.*, p. 281.

causality involved therein, be wholly identified (1) with the chemicals and energies of the organism so acting; (2) with the organization or relations of these chemicals and energies; or (3) with any selection from these chemicals and energies, such as Montague's, Osborn's and other physicochemical hypotheses of the particularistic type appeal to?

Psychical, or Non-Physical, Factors in Animal Behavior

Let us proceed at once to an examination of this question. We shall leave to one side the well worn arguments adduced by adherents of the various positions on this question, and for the reason that they do not seem capable, judging from the continuation of the controversy thereon, of yielding any definitive conclusions with regard to it. Whether or not that be true, it should be more interesting, and perhaps more profitable, to seek for some fresh evidence on this issue.

Our argument, in brief, will be that inference, meaning, judgment and other cognitive processes, together with feelings, emotions, desires, memories, anticipations and purposes, constitute genuine causal factors in the behavior of organisms endowed with these attributes, and that these causal factors cannot be completely identified with any or all of the chemicals and energies constituting the organism on its physical side. We will begin by indicating some features in the genesis of certain types of meaning, inference and judgment, the more significant components of the alleged causal factors in question. Let it be remembered that, in this discussion, chemicals, energies, bodies, physical objects, etc., refer to "sensory elements" or physicochemical entities of all sorts, exempted from any particular epistemological interpretation of these entities. More positively, these terms here mean the objects and processes investigated by the physicochemical sciences and recognized as in some sense real by all schools of philosophical thought.

We shall develop our analysis with the aid of familiar examples. A morning newspaper, a certain complex of sensory elements, sets up various processes in my body, and inferences or judgments arise respecting physical objects and events at, say, the antipodes. These inferences, judgments and other cognitive processes thus establish some sort of relation between widely separated objects in space. By like processes similar relationships are established between the organism and physical objects or events existing at some past time, or between an existing body and an object or event no longer existent. Similar relationships are likewise established between present objects or events, and future but not yet existent objects or events. Evidently there are in all such cases implications associated with one group of objects

and events, immeasurably transcending, in their spatio-temporal reference, those objects and events themselves.

How will various types of the physicochemical hypothesis account for the facts thus barely indicated, leaving now to one side the question whether these self-transcending implications have any casual significance? Any physicochemical hypothesis which includes as one of its doctrines some strict form of psychophysical parallelism can admit these facts without embarrassment, provided certain assumptions be allowed it, such as that impotent series of psychical events may exist and need not be specially accounted for. For so far as is known definitely, every psychical event is accompanied by a physical event, and it can be plausibly claimed that the physical events presumably correlated with psychical processes are the sole determinants of relationships between the organism and widely separated objects, and not the psychical processes so intimately concerned, apparently, in such relationships. This applies to relationships of the organism with objects and events remote in time as well as with those distant in space, since, even if there is not an unbroken series of physical events between the present, and past or future, present physical events might, as Professor Montague claims, imply past or future events.

A similar analysis applies to two-aspect versions of the unity doctrine, which make the physical but not the psychical aspect of the psychophysical a truly efficacious one, provided, in turn, the necessary assumptions be allowed it, particularly the assumption that the physical and the psychical *can be* identified in a single entity or type of entities. Likewise, particularistic physicochemical conceptions, such as Montague's, can also find a place for the same facts, provided they be allowed the assumptions severally required by them as, for example, in Montague's doctrine, that potential energy and consciousness can be identified in one type of entity (whether qualitatively or parallelistically), and that the type of causality associated with consciousness can be attributed to some entity or class of entities of an energetical type. The auxiliary assumptions required by these several conceptions are of doubtful validity, as we shall see later, but these do not bear directly on the analysis at this point.

It is different with forms of the physicochemical hypothesis which make psychical processes epiphenomena, functions, emergents or relations of the correlated physical processes. For since the latter on all theories of knowledge are sensory elements, *i. e.,* are constituted, represented or expressed by *sense*-impressions, it is only by an arbitrary assumption that inference, judgment and other psychical processes, which are or contain elements radically heterogeneous, qualitatively,

with sense-impressions, can be deduced from the latter or their efficient elements. We have, however, elaborated this criticism elsewhere and need not consider it further at this point.

Let us consider now the question whether the relationships between organisms and other physical objects, that are constituted or established by cognitive processes have, as such, any influence on the behavior of organisms and, if so, whether such influence is to be identified with any or all of the physicochemical processes of the organism or other parts of the given physical system.

I infer from reading a letter that a long-lost friend or a much coveted position awaits me in a city five-hundred miles distant. I decide to go there, make the necessary preparations, plan my itinerary and in due course of time arrive at the city in question. This series of phenomena involves a great number of chemicals and energies, related and interacting in complex and changing ways; but the whole series of phenomena was determined, in its broad outlines, as soon as my decision was made. There are involved, besides this physical series, memories, concepts, meanings, inferences, judgments, feelings, emotions, desires, anticipated future events (images), volitions, etc., as well as the sensations (or sensory elements) correlated with and partially constitutive of these psychical processes, or otherwise involved in the realization of my purpose. Can the chemicals and energies (or sensory elements) involved in this series of phenomena account for its origin and order? In other words, are the sensory elements the exclusive causal factors in this situation?

We believe it probable, were our knowledge sufficient, that the *amount* of energy utilized or transformed in this series of phenomena could be wholly accounted for in terms of these sensory elements and, of course, that the bodies involved therein could be similarly accounted for. Indeed, our neutral conception of sensory elements obviously carries this implication. The present instance is in fact only a specific illustration of that conception. We believe further, and for the same reasons, that all the specific directions taken by the component bodies of this system could be similarly accounted for. But the origin and order of my movements toward their distant destination cannot be accounted for in similar terms. My movements, their origin and order are the significant features of this situation. These must be wholly interpreted, on any physicochemical conception of my organism, in terms of energies combined in my body or utilized by it.

Now, for virtually none of the distance travelled on my journey, could any sort of energy associated with my body, or with the distant city, constitute an effective energetical bond between them. Let all

the resources of the chemical and physical sciences be brought to bear in estimating the amounts of electromagnetic and other types of energy emanating from my body and from objects in the distant city, and such an estimate will afford no basis for the belief that any bond of an energetical type draws me to that city. But that *something* is drawing me there cannot be questioned. I am being attracted to a certain position on the earth's surface as effectually as the magnetic needle is attracted toward the North Pole (or thereabouts), but in part by factors of a totally different character from those exemplified in the latter and similar instances. The fact that not all human purposes are realized does not argue against the causal efficiency of these purposes or the non-physical factors identified with them, any more than failure of many magnetized substances to orient themselves with reference to the lines of force in the given magnetic fields argues against the causal efficiency of the latter. Failure in both cases merely signifies the intervention of frustrating or counteracting factors, besides illustrating differences in the types of causal factors involved in the two cases.

It is unnecessary to multiply illustrations of this non-physical or psychical type of causality. It is indeed involved wherever the organism (notably, of course, the human organism) acts with reference to other bodies, including other organisms, not immediately related to the given organism by an effective energetical bond. That such action includes all behavior directed with reference to anticipated future events should be clear from a consideration of our typical instance and its similarity to the activities of daily life.

This type of activity largely rests on a social basis, of course, since we generally act with reference to similar actions of other human beings, which in turn have the same sort of reference to ourselves or to others so related to ourselves. Moreover, our actions proceed on the basis of tested knowledge, in the development of which a host of individuals, both living and dead, have been concerned; and of hypotheses and beliefs accepted as valid or necessarily reserved for future testing, in the formulation and eventual verification of which many other individuals have been or will be involved. Thus, my journey to meet a friend or assume the duties of a position in a distant city involves inferences, meanings, judgments, hypotheses, verifications, both of mine and of others, integrated in a complex system of interacting elements, and involving knowledge, ideals and purposes socially conditioned in a broader sense, and indeed continuously developed over a long period of human history.

By similar considerations grounded on the imputed mental affinities of other animals with ourselves, most if not all phases of animal be-

havior could be brought under the same general analysis. This topic we do not wish to go into, however, because our contention will have been established, if established at all, through the analysis of human behavior; and because, also, our limited acquaintance with the cognitive processes of other animals makes any affirmative conclusions in this field extremely difficult, and perhaps impossible at the present time. We will say, however, that at least one negative, together with a corresponding affirmative, conclusion as to this matter seems amply supported by the facts of animal behavior. The origin and order of an animal's movements toward an object which does not attract that animal in an energetical sense hardly seem explicable in terms of the energies at work in this situation. Both the animal *and* the object signify causal facors in this series of movements; and both are therefore integrated in one system of interacting causal factors; but the energies involved in this system—not excluding the sensory stimuli originating in the object—do not in a physical sense effectively bridge the gap between the animal and the object to which it reacts. This negative conclusion, if valid, implies that causal factors of a non-physical type are involved in this situation. The inference seems legitimate, considering the imputed mental affinities between ourselves and other animals, that this non-physical causality is psychical in character. This pair of conclusions—negative and affirmative—obviously applies to all phases of animal behavior definitely related to external objects with which the given animal is not, in the initial stages of its behavior, in effective energetical relations. These include *all* animal behavior, properly so-called. Whether or to what extent a similar conclusion would apply to the activities of plant organisms we shall not here consider. Discussions elsewhere in this volume support the proposition, however, that causal factors of a non-physical type are involved in plant activities.

Some observations respecting the importance of non-physical factors in human behavior may be introduced. These are obviously very farreaching in their influence. Besides guiding our action with reference to the immediate environment, these factors are to be given the primary credit for the more or less deliberately directed interchange or redistribution of inanimate bodies, plant and animal organisms, human knowledge, beliefs, traditions, institutions, customs, arts and purposes, over the earth's surface; and, primary credit, also, for the development of facilities whereby the transportation or communication of all these things is effected. That material bodies and energies are involved in every particular of this interchange cannot of course be questioned; but, on our analysis, the origin, order and system of such interchanges,

whether taken distributively or collectively, are not attributable solely to any or all of the physical factors involved therein. That this group of factors largely determine our complex modern society or, more specifically, the characteristics whereby human life to-day is differentiated from inorganic activities, should be obvious. Equally obvious should it be that human activities are differentiated from the activities of other animals by our possession of non-physical attributes in much more complex and potent forms than characterize other animal species.

If our analysis up to this point contains no fatal errors, therefore, a type of non-physical causality has been demonstrated, and a most important field for it delimited. This analysis carries critical implications for all doctrines regarding the mental and the physical which deny to mental factors a causal influence on physical events; or which admit such causal influence in this domain, but identify the mental with some or all of the chemicals and energies correlated with it in the organism; or, again, which, while admitting empirical differences between the mental and physical, put these down as differences in the combinations, contexts or manifolds of neutral entities alleged to constitute both the mental and the physical. Let us briefly indicate these critical implications of the analysis.

That the analysis applies, with destructive consequences, to all forms of the physicochemical hypothesis should be sufficiently indicated by the fact, already stressed, that any such hypothesis must account in energetical terms for the origin and order of animal movements directed toward distant objects, with which the given animals are not effectively related, in an energetical sense. This statement should be adequate, as it stands, in its application to (1) all versions of the elementalist hypothesis, which allege that the causal factors of the organism lie exclusively in its specific chemicals and energies; (2) all epiphenomenalistic and parallelistic doctrines of the mental in its relation to the physical; and (3) all particularistic forms of the physicochemical hypothesis interpreting the peculiar properties of the organism in terms of particular chemicals or energies actually or by hypothesis present therein.

Some elaborations of our analysis with respect to (1) organismic forms of the physicochemical hypothesis, (2) particularistic conceptions of the type proposed by Montague, and (3) unity doctrines of the psychophysical will, however, be in order. These can be indicated very briefly.

On organismic forms of the physicochemical hypothesis, the mental as well as other vital properties of the organism, so far as these are causally significant, are relations or qualities of the chemicals and en-

ergies of the organism, as organized therein. While these relations or qualities are emphasized in organismic interpretations, they are nevertheless regarded as physicochemical in character, as products of physicochemical activity, and interpretable, without residue, in physicochemical terms.

Reserving for separate consideration the question whether the system of relations between chemicals and energies in the organism can be accounted for in physicochemical terms, we may focus attention on the question whether the relations which *are* physicochemical could constitute the mental factors in human action with reference to distant objects. Our answer would be that such action, according to the foregoing analysis, is characterized by features having *nothing* in common with any known type of physical or chemical action. My action with reference to future events in a city a thousand miles distant is not, as such, physical or chemical in character, nor does it *remotely* resemble action of this character. The distinctive features of action with reference to a remote space or time differ as much from any known type of physical or chemical process as visual sensations differ from kinaesthetic. They are radically heterogeneous in character. And there are not the slightest grounds, so far as we can see, for assuming that any possible combination of chemicals and energies could produce something totally unlike themselves, just as, recurring to our analogy, no possible combination of visual sensations could ever produce kinaesthetic sensations. The two things differ *toto caelo,* if mundane things can ever differ by so much. But organismic forms of the physicochemical hypothesis, by assumption, attribute all the peculiar properties of the organism to its system of physical and chemical activities. Therefore, if action with reference to a distant space and time involves factors or properties which cannot possibly be derived from chemicals or energies of any sort, these as well as other forms of the physicochemical hypothesis are clearly untenable.

These same considerations apply, with but little modification, to the energistic conception of the psychical advocated by Montague and others. If human action is partially determined by factors that have nothing in common with energy, a genuine energistic conception of human action, particularly of the psychical factors therein, is clearly ruled out. True, Montague makes his potential energy, anergy, etc., behave as do these psychical factors, but, on our showing, such entities (if there be any such) are either not energy, or else not psychical. The conclusion is inescapable that Montague seeks to identify two groups of factors that are of radically heterogeneous kinds. With this hybrid conception must evidently go his conception of conscious-

ness as the self-transcending implications of objective (meaning physical) events; and part at least of his doctrine of causality, according to which cause-effect potentiality is the consciousness of other events, or the implications of absent physical events by present physical events, especially brain processes. The type of causality given by inference, judgment and other cognitive processes does not, according to our analysis, conform to these specifications.

The unity or two-aspect doctrine of the psychophysical also falls under the criticism implied by our analysis. The many versions of this doctrine need not be specified and criticized in detail. The two-aspect doctrines of Warren and Sellars have already been cited. Montague, in one of his moods, entertains a two-aspect doctrine of psychical and brain processes, though seeking at other times to indentify they qualitatively. Strong, in a well known book,[1] expounds a doctrine termed psychophysical idealism, according to which brain processes (or certain of them) are the (potential) phenomenal appearance to an outside observer, of the consciousness alleged to be identified with those processes, and the *reality* behind such phenomenal appearance. These conceptions will serve to exemplify the general doctrine under consideration. It should be noted that Strong's doctrine is, nominally at least, the converse of the other unity doctrines cited, though, in effect, it implies, as they do, causality of a physical type,[2] despite its insistence that the phenomenal sequences termed physical symbolize things-in-themselves which *are* consciousness, or in the nature of consciousness.

Now, whether consciousness be correlated with the brain, the entire nervous system, or the body as a whole, it is shown, by our analysis, to be or include something over and above any complex of physical elements with which it may be correlated. The most liberal estimate of the possible energy manifestations of such a complex will not provide a basis for identifying them completely with the series and order of the organism's movements toward objects or events remote in space or time. For there *are* the bodies and movements in this system which physics and chemistry analyze for us, albeit incompletely; and there are the movements of the organism toward distant objects, observable in all human activity, and intimately correlated with processes of consciousness, analyzed, also incompletely, by the logician and the psychologist; and the two series are so disparate, that there is no warrant for

[1] *Why the Mind Has a Body.*

[2] *Op. cit.*, pp. 343–345. The mechanistic implications of Strong's doctrines are more pronounced in his later book, *The Origin of Consciousness*. Cf. Pratt, J. B., *Matter and Spirit*, p. 22.

their complete indentification in one entity or type of entities, whether as two correlative aspects thereof, or as reality and its appearance. The disparateness must be put down to the genuine causal influence of mental processes associated with the organism's behavior and irreducible to terms of its physicochemical activity.

The Radical Empiricist's Conception of the Mental

We may now indicate the critical implications of our analysis for behaviorism, radical empiricism and relational theories of consciousness, which either imply a physicochemical conception of the organism and its behavior, or repudiate the ontological distinction between physical and non-physical factors therein upon which we have insisted. We shall first discuss radical empiricism in this connection, confining ourselves to those features of the doctrine, particularly as formulated by James and Holt, which are in opposition to the type of theory here set forth.

According to this doctrine, there is no valid disjunction between consciousness and physical nature, but "only differences of empirical relationship among common empirical terms"; [1] for both are composed of the same sort of stuff, termed by James pure experience, by Holt neutral entities.[2] The relational differences, according to James' account, however, are pretty substantial. While perceptual experience is said to figure "in one context as an object or field of objects, in another as a state of mind," [3] and without any self-diremption of consciousness and its content, objects and mental states are different in that real consequences always accrue to the former, but not to the latter. Real fires will always burn, real water always quench a real fire, and so on, whereas "mental fire is what won't burn real sticks; mental water is what won't necessarily (though of course it may) put out even a mental fire." [4] In general, " 'outer' and 'inner' are names for two groups into which we sort experiences according to the way in which they act upon their neighbors . . . the basis of the two groups respectively is the different type of interrelation, the mutual impenetrability, on the one hand, and the lack of physical interference and interaction, on the other." [5] "The real experiences get sifted from the mental ones, the things from our thoughts of them, fanciful or true, and precipitated together as the stable part of the whole experi-

[1] James, *Essays in Radical Empiricism,* p. xi.
[2] *The Concept of Consciousness, passim.*
[3] *Op. cit.,* p. 18.
[4] *Ibid.,* pp. 32–33.
[5] *Ibid.,* pp. 139–140.

ence-chaos, under the name of the physical world. Of this our perceptual experiences are the nucleus, they being the originally *strong* experiences." [1]

Continuing, James speaks of the physical world as the core of reality around which "the world of laxly connected fancies and mere rhapsodical objects floats like a bank of clouds." Although in these and other passages James seems to regard physical contexts of experience as of a higher order, existentially, and more efficacious, causally, than the mental, he earlier argued cogently for the efficaciousness of consciousness,[2] and, so far as we know, has never recanted this view of the matter. We need not go further into James' explicit pronouncements as to the causal significance of consciousness, but will concentrate our criticism on one of the cardinal doctrines of his radical empiricism, namely, that consciousness and the physical world are but the same stuffs of experience taken in different contexts or empirical relationships. This may be reserved, however, until Holt's version of the same general doctrine has been inspected.

Holt differs from James mainly in the emphasis he gives to neutral entities of logical and mathematical types; in his tentative formulation of a hierarchical order of complexities by which these neutral entities are graded; and in his more explicit conceptions of the living organism and of consciousness, respectively, and their positions in this hierarchy.

His hierarchical order of complexities starts with the relatively universal (which includes logical and mathematical entities), as the most simple and fundamental components of the various complex entities; and proceeds thence to the "innumerable algebras," the secondary qualities, space, time, motion, mass, energy and so on until the values treated by the normative sciences are reached as highest in this order of complexities.[3] Consciousness or mind is said to appear about midway in this simple-to-complex order of being.[4]

The more complex systems are always some of the simpler ones, but with an additional determination. But the hope is entertained that such additional determinations will be found reducible to entities of the simpler sort. However that may be, Holt boldly affirms that life is "some sort of chemical process, and nothing further;" [5] and that no new substance enters into consciousness, that "the consciousness aggre-

[1] *Ibid.*, p. 33.

[2] "Are We Automata?" *Mind*, Vol. IV, 1879, pp. 1–22; *Principles of Psychology*, Vol. I, pp. 136 ff.

[3] *Op. cit.*, pp. 154–160.

[4] *Ibid.*, p. 166.

[5] *Ibid.*, p. 158.

gate can be readily and completely defined in terms of the entities that have appeared before." [1] The cross-section of the environment (a highly selected complex of neutral entities) to which the organism responds is its consciousness, mind or soul; and sensations, perceptions, ideas, etc., are the individual members of this cross-section. [2] The manner in which such an environmental cross-section is defined by response of the organism constitutes the knowing process. [3] Elsewhere it is declared that "both minds and physical objects are and are 'real'"; and that "they are composed of one and the same substance—neutral stuff." [4] This neutral stuff includes colours, sounds, smells, motions, energies and masses, which are all immediate objects of our experience. [5] Even mathematical concepts, which are neither mental nor physical, "enter into the construction both of minds and of objects." [6]

We may for our purposes dispense with further details of this doctrine, [7] to which, it need not be said, so summary an account as ours scarcely does justice. Our critical comment on the doctrine will be even briefer than the statement of the doctrine itself, as the implications of our analysis for it are so obvious that a detailed development thereof will not be necessary.

The differences recognized by James between consciousness and the physical world are so significant that it is difficult to see how he could suppose them to be composed of the same neutral stuff. For not only is the physical world made much more stable and efficacious than consciousness, but specific stuffs of experience entering therein, and said to be the same, behave very differently in the two contexts. Fires that won't and will burn, water that won't and will quench a real fire, knives that won't and will cut real objects are respectively so different that to assert their sameness is clearly a contradiction. Holt does not seem guilty of this particular contradiction.

But the analysis developed in this chapter carries destructive consequences, as we think, for both James' and Holt's versions of radical empiricism. For physical manifolds with psychical attributes added to them behave so differently, as we have shown, from physical manifolds without such attributes that the conclusion is inescapable that the former includes a type of entity which is not also to be imputed to the latter. The same statement applies to *purely* physical and *purely*

[1] *Ibid.*, p. 159.
[2] *Ibid.*, Chap. IX, esp. pp. 182, 183.
[3] *Ibid.*, p. 179.
[4] *Ibid.*, p. 124.
[5] *Ibid.*, p. 122.
[6] *Ibid.*, p. 133.
[7] Some additional details are cited on pp. 188–190, 194–195, 273–276, *supra*.

psychical manifolds, if there are any such contexts of "experience," which, however, we shall find reasons to doubt, in our next chapter. This application of our analysis rests not only on our proof that consciousness connotes a non-physical type of causality, but also on the proof that consciousness is irreducible to relations, emergent qualities or other resultants of purely physicochemical processes.

Relational Theories of Consciousness

Both these versions of radical empiricism are instances of what has been termed the relational theory of consciousness. On this form of the theory, however, consciousness, or a psychical manifold, is not constituted by relations alone, but by relations of a particular kind *and* the terms of which they are relations.

Another type of relational theory deprives mind or consciousness of all terms, and identifies it exclusively with relations of a particular type between terms, namely, logical relations. According to this doctrine, all other relations and qualities are referred to non-psychical objects, the logical relations between which constitute consciousness. Consciousness is thus a particular sort of relations, functions or actions which non-psychical objects manifest or enter into. This view implies, when it does not explicitly affirm, a neo-realistic theory of the objects entering into consciousness, or systems of logical relationships.[1] We may add that it implies a physicochemical conception of the organism and of consciousness, since the objects it conceives (explicitly or implicitly) in a neo-realistic sense are of the type which constitute the subject-matter of the physical and chemical sciences.

Professors Dewey and Woodbridge advocate this type of relational theory, and a brief examination of their expositions thereof will serve to elaborate the appraisal of it just indicated.

A recent article by Dewey contains so succinct a statement of the theory as he conceives it that we may reproduce it in his own words. This theory "starts with a thing, *res,* actually present, smoke, rock, and with the present fact that this something refers to something else of the same order of existence as itself, a fire, or geologic animal. It bases itself upon the undoubted occurrence of inference from one present thing to another absent thing of the same non-psychical kind. It thus avoids the breach of continuity, the dualism, involved in dividing existence into two orders, physical and psychical, which are defined only by antithetical attributes, and of such a nature that reference and

[1] Montague, W. P., "The Relational Theory of Consciousness and Its Realistic Implications," *Jour. of Phil., Psych. and Sci. Meth.,* Vol. II, 1905, pp. 309–316.

intercourse between them is an affair totally unlike any other known matter. It also has the advantage of starting from a *vera causa,* the undubitable fact of inference.

". . . That one objective affair should have the power of standing for, meaning, another is the wonder, a wonder which as I see it, is to be accepted just as the occurrence in the world of any other qualitative affair, the qualities of water, for example. But a thing which has or exercises the quality of being a surrogate of some absent thing is so distinctive, so unique, that it needs a distinctive name. *As exercising the function we may call it mental.* Neither the thing meant nor the thing signifying is mental. Nor is meaning itself mental in any psychical, dualistic, existential sense. . . . Such a theory, it will be noted, explains the mental on the basis of a logical function. It does not start by shoving something psychical under a logical operation." [1] Elsewhere in the same article Dewey gives his theory the name of pluralistic realism. "Things are things, not mental states. Hence the realism. But the things are indefinitely many. Hence the pluralism." [2]

Dewey refers, for elaborations of this theory, to various sections of his *Essays in Experimental Logic.* [3] A detailed presentation of these elaborations may for our purposes be omitted. Only certain features thereof need be indicated. Meaning is said to be always a function of the situation; *a* meaning, however, is not a function, but a specific entity, *e. g.,* the man *as* suggested by a footprint, etc.[4] Knowledge goes on in terms of things, not of relations between things and a peculiar existence called consciousness. Thought or intelligence is defined by "function, by work done, by consequences effected," not as " an entity or substance or activity which is ready-made thought or reason." It is a "name for the events and acts which make up the processes of analytic inspection and projected invention and testing." These events and acts "comprise the sticks and stones, the bread and butter, the trees and horses, the eyes and ears, the lovers and haters, the sights and delights of ordinary experience. Thinking is what some of the actual existences *do.*" [5] Inferential knowledge or reflective thinking, thus conceived, is assimilated to "a certain mode of readaptation of functions.

[1] "Realism Without Monism or Dualism—II," *Jour. of Phil.,* Vol. XIX, 1922, pp. 357–358. Footnotes in the passage cited are omitted. One of these states that the dualistic theory of the mental encounters more difficulties than other theories "because of the implied breach of continuity."

[2] P. 356.

[3] Pp. 136–156, 220–229, 430–433. See also the "Introduction" to the same volume, esp. pp. 14, 31.

[4] E.E.L., pp. 431–432.

[5] *Ibid.,* p. 31.

involving shock and the need of control"; while sensation, perception, image, emotion, concept, "mean peculiar (*i. e.,* specifically qualitative) epochs, phases, and crises in the scheme of behavior." [1] This of course is the pragmatic doctrine of knowledge in one of its forms, but this doctrine is not necessarily implicated in the relational theory of consciousness, and need not be considered in connection with the latter.

Nowhere does Dewey offer a "proof," so far as we know, that his relational theory of the mental is valid as against dualistic or other theories at variance with it. So far as the grounds are indicated, in the works cited, for his rejection of the dualistic theory (which may be taken to approximate, roughly, the position herein defended), these seem to lie in the difficulties said to be implied by the breach of continuity, and by the interaction between two antithetical orders of existence, assumed by dualism. These dualistic doctrines create an artificial problem of knowledge which must be cleared away before a valid conception of knowledge, as illustrated by the sciences, for example, can be generally received.

These matters we need not go into here, but may remark that for one physical thing to mean another physical thing remote from the former in space and time and not effectively related with it in a *physical* sense, seems to present difficulties at least as great as those said to be encountered by dualism. These difficulies are accentuated by the fact that one physical thing means another thing of the same sort only *for* a third thing, an organism, which appears to have some "determinations" not found in other sorts of physical things. Meanings therefore would appear to depend on a thing differentiated from all other sorts of physical things, by some additional determinations, and therefore scarcely to pertain to physical things as such. Fire, smoke and fuel taken by themselves, therefore, do not *mean* each other; and the same statement applies to any combination of two or more things whose exclusively physical character is not subject to debate.

The development of these considerations, however, would involve the implications of our previous analysis for this doctrine, and the statement of these may be reserved until Woodbridge's version of the same doctrine has been reported. We would only say, in addition, that the ground of Dewey's objections to the dualistic theory of consciousness seems to lie in his acceptance of the old axiom that unlike things cannot interact or enter into a single, but complex order of existence, wherein they are essential factors. This axiom, as we shall see later, has long been abandoned in the physical sciences and indeed seems to be violated by most types of interactions actually found in nature. So

[1] *Ibid.,* pp. 221–222.

much by way of comparing the more obvious difficulties (real or alleged) of the two types of theory here brought into opposition.

Woodbridge's theory of consciousness agrees in its main outlines with that of Dewey, and our exposition of it may therefore be a brief one.[1] Woodbridge holds, with Dewey, that consciousness is not something distinct from its objects, but is a type of relations between objects, namely, logical relations. The fact of meaning is the fact of consciousness; and consciousness is the existence of logical reasons.[2] The objects of consciousness also enter into other types of relations, notably spatial, temporal and individual-class relations. And the objects of consciousness are no more determined by the fact that they are in consciousness, than objects in space and time are determined as to their specific qualities by their spatial and temporal relationships. These are all analogous, though radically different, continua of relationships into which the same objects enter, and without affecting the nature of the latter. Knowledge is therefore palpably realistic: secondary qualities are not part of consciousness; and the world is not transformed into ideas, but only illuminated and connected up in a new way by consciousness. Consciousness does not mean that ideas represent things, that phenomena represent noumena, that states of conciousness represent the external world, etc. Awareness, a prime characteristic of consciousness, is only the manifold and irresistible meaning connections between things in the conscious situation.

Consciousness is given an essentially physical interpretation in this doctrine. The body is said to be the mind's essential basis and support, while the mind is the body's perfected operation and achievement, as Aristotle taught. Consciousness is the outgrowth of evolutionary reorganization, wherefore there is no ground for the assumption that it signifies the intervention, in evolution, of an entirely new order of existences.[3] It is therefore difficult to take the problem of interaction seriously, and meaningless to ask the question whether consciousness is efficient. Conscious bodies are obviously more efficient than nonconscious ones, just as a live man is more efficient than a dead one.[4] That a conscious being thinks of things remote, in time and space,

[1] Woodbridge's theory is set forth in the following series of papers: "The Nature of Consciousness," *Jour of Phil., Psych. and Sci. Meth.*, Vol. II, 1905, pp. 119–125: "The Problem of Consciousness," *Studies in Philosophy and Psychology*, by former students of Charles Edward Garman, 1906, pp. 137–166; "Consciousness, the Sense Organs, and the Nervous System," *Jour. of Phil., Psych. and Sci. Meth.*, Vol. VI, 1909, pp. 449–455; "Mind Discerned," *Jour. of Phil.*, Vol. XVIII, 1921, pp. 337–347.

[2] "Consciousness, the Sense Organs, and the Nervous System," p. 449.

[3] "The Problem of Consciousness," pp. 152–153.

[4] *Ibid.*, p. 152.

from his organism is, however, a unique and baffling kind of fact.[1] Yet, while we do not know why the interaction of the organism with its surroundings produces consciousness, this is only a special case of our ignorance about the organization of the world in general.[2]

Although the meaning connections which constitute consciousness are relations between material objects (and the mind is the perfected operation of the body), those relations are immaterial in character, and by them are constituted an immaterial synthesis of material objects. Other types of relations make possible material syntheses of the same objects.[3]

Two positive arguments (perhaps others) are advanced in support of this doctrine. One is that "consciousness is never discovered as one thing set over against other things which are not already its content"; and that, consequently, "it seems futile to suppose that it is, and then proceed to build up a theory about it." [4] We may comment on this argument, before considering the second argument. It can be readily shown, we think, that it is by no means a conclusive one. It apparently overlooks the possibility that entities of one sort may always be combined with entities of a quite different sort, and yet that the two types of entities thus combined may be distinguished, not by their empirical separation, which, by hypothesis, is impossible; but by an *analysis in situ,* as, in the present case, by comparing the actions of bodies with and without consciousness, respectively. A closely analogous case is the functioning of hereditary traits, without exception, in relation to the surroundings of the organism; whence it is *not* concluded by the biologist that heredity does not stand for real existences distinguishable from environmental conditions, but, on the contrary, that it does stand for such existences.

The second argument is more involved, and we may not rightly have grasped its import.[5] It turns on the distinction between mind in the transcendental sense, as including the whole universe of discourse, and mind in the more restricted sense of that which inhabits animal bodies and constitutes the subject-matter of psychology.[6] One contention of the paper cited is that mind in the transcendental sense does not depend

[1] *Ibid.,* p. 152.

[2] "Consciousness, the Sense Organs, and the Nervous System," *Jour of Phil.,* Vol. VI, 1909, p. 450, footnote, quoting Dr. Adolf Meyer.

[3] "The Problem of Consciousness," p. 160.

[4] "The Nature of Consciousness," p. 119.

[5] "Mind Discerned," *Jour. of Phil.,* Vol. XVIII, 1921, pp. 337-347.

[6] While this distinction is borrowed from idealistic metaphysics, it is utilized only for purposes of elaborating a theory of mind, in the restricted sense, and its relation to the remainder of the universe; the ultimate validity of the conception of mind, in the transcendental sense, is not affirmed.

on mind in the restricted sense. With this we are not here concerned.

The argument in support of the relational theory of mind (in the restricted sense) is to the effect that mind in this sense is dependent on mind in the transcendental sense. With the idealistic epistemology dropped out, the argument means that mind is dependent (in the ontological sense) on the physical conditions wherein it appears, and with which it is always conjoined. A more special formulation of the same argument states that animal bodies cannot be exhibited without any implication of mind, from which it is inferred that mind is a function of animal bodies. An analogy is drawn between operations of the mind —thinking, remembering, etc.—and digestion, respiration and other physiological processes; whereupon it is argued that there are no grounds for putting mental operations in a class apart from other functions of the body. It is affirmed that "thinking, like digestion, is a reaction to a world congenial to it." [1] The fact that only animal bodies think about the world does not put them in a different universe from other parts of this world, since it is not discovered that the animal body as an object of study differs at all from other objects studied by animal bodies.[2]

This argument depicts an intelligible and in many ways plausible conception of the mind and its relation to the world in which it appears; but it rests, like the first argument, on the assumption that if one feature of reality is always found conjoined with a different feature, the two must be identified ontologically; and this assumption, as we have seen, may be questioned in any given case, since it does not always hold true. Also the proposition that the animal body is not different from other objects studied, along with it, by the animal body itself is only a statement of *one* controversial position on this problem, which, with rival positions, is still *sub judice*. The evidence presented in this and the following chapters cannot, we believe, be squared with this proposition.

We may now develop the implications of our previous analysis, with reference to the relational theory of consciousness expounded by Dewey and Woodbridge. That analysis resulted in a demonstration that a type of causality is involved in animal behavior, which is irreducible to terms of energy or any other sort of physical entities. This type of causality was of course identified with psychical attributes of the organism, including meaning (stressed by Dewey and Woodbridge) and other cognitive processes. Now, while both these philosophers fully recognize the efficacy of those attributes of the organ-

[1] *Ibid.*, p. 346.
[2] *Ibid.*, p. 344.

ism, they are conceived as relations between *physical objects*. Consciousness does not, on their theory, imply terms other than these physical objects. Since relations implicate the terms which they relate, and only physical terms are recognized, the relations as well as their terms must belong to the physical world. The relations constituting consciousness must therefore be physical relations. Now, our analysis has shown that action of the organism with reference to objects or events remote in space and time involves relations which are *not* physical, though involving physical relations as well. The analysis therefore refutes this type of relational theory as we have construed it.

But Woodbridge (and perhaps Dewey) hold that the logical relations which constitute consciousness are *immaterial* reations, although relations between physical objects. If by this is meant that logical relations are non-physical ("immaterial" might have a different meaning), then it seems clear that the conception is a self-contradictory one. For, on this conception, the logical relations constituting consciousness have no other source or center than the physical objects which they relate; and we must have physical objects generating or giving rise to non-physical relations; or, if the relations be deemed more fundamental than their terms, as is logically possible, we should have non-physical relations constituting or synthetized in physical objects. Either, then, the relations pertaining to the same objects are of fundamentally different types, and something *besides* the physical objects themselves must account for this difference in relations; or, if relations be stressed as fundamental, relations of radically different types could not at different times constitute or be synthetized in the same objects, but must represent different objects or entities. The relational theory of consciousness in this form must apparently yield, therefore, to a theory of consciousness as constituted by terms and relations of a non-physical type, though entering into complex systems with physical terms and relations.

Behaviorism

That our analysis, if sound, refutes all forms of behaviorism, when it means not alone a type of method for investigating behavior, but a theory as to the nature of behavior, is so clear that we scarcely need call attention to the fact. Behaviorism in the sense here considered professes to account for animal behavior exclusively in terms of body movements with reference to specific features of the environment, including the reactions of body parts to their environment. Thought is

defined either as the sum total of all these reactions or, quoting a leading behaviorist, as "the labile interplay of motor settings which goes on almost constantly, and which differs from overt conduct in that the energy involved is too small to produce gross bodily movements." [1]

Although behavioristic doctrines of thought or consciousness are not always explicitly mechanistic in intent, it is clear that they all imply a mechanistic conception of the organism, since the *bodily* structures and responses, together with the objects eliciting response, which are alleged to determine behavior, are all conceived as physical objects or processes. There may be, as in Holt's case, a denial of any valid ontological distinction between consciousness and physical objects, as both are said to be constituted by the same sorts of neutral entities; but his consciousness turns out to be just those objects of the environment (as defined by the neural response of the organism) which constitute the subject-matter of the physical sciences, while life is explicitly defined by him as some sort of chemical process, and nothing further.[2] Likewise, Dewey cannot, without a construction of his relational theory of the mental, be characterized as a physichochemicalist, since the logical relations which for him constitute the mental are not generally conceived to be physical in character; but when these relations are found to subsist only between physical objects, we are obliged to conclude either that his theory is a physical, or else a self-contradictory, one.[3]

We may say, in any case, that behavioristic doctrines which interpret the activities of the organism exclusively in terms of the *body* and the physical objects to which it responds are refuted by our previous analysis, since this analysis showed that animal organisms, notably

[1] Holt, E. B., *The Freudian Wish*, p. 94. Holt's theory is elaborated in *The Concept of Consciousness*, and "Response and Cognition," *Jour. of Phil., Psych, and Sci. Meth.*, Vol. XII, 1915, pp. 365–373, 393–409 (reprinted in *The Freudian Wish*). Watson defines thought, on different occasions, in each of the two ways indicated above. Compare "Is Thinking Merely the Action of Language Mechanisms?" *British Jour. of Psych.*, Vol. XI, 1920, pp. 87 ff., with *Psychology from the Standpoint of a Behaviorist*, p. 325. Somewhat different behavioristic definitions of thought are to be found in Dewey's "Knowledge and Speech Reaction," *Jour. of Phil.*, Vol. XIX, 1921, pp. 561–570, and *Essays in Experimental Logic, passim;* and in Mead's "A Behavioristic Account of the Significant Symbol," *Jour. of Phil.*, Vol. XIX, 1921, pp. 157–163. Copious references to behavioristic literature are given in Roback's *Behaviorism and Psychology*. The literature of behaviorism, *pro* and *con*, is of course very extensive, and a detailed appraisal of it is not here intended, because unnecessary to our purposes.

[2] *Supra*, p. 225. It should be noted that, on Holt's theory, thought is something entirely different from consciousness. Both, however, are physical, on his theory.

[3] The justice of this criticism depends of course, on the validity of the analysis whereby we have attempted to evaluate the relational theory of mind held by Dewey and Woodbridge. *Supra*, pp. 227–233.

human organisms, act with reference to objects and events not related, in a physically *effective* way, with the acting organisms. The conclusion of course was that causal factors of a non-physical type are involved in animal behavior.

It would be of interest to evaluate from the standpoint of our analysis, dimensional and other theories of consciousness not hitherto dealt with, did space limitations permit.[1] The implications of our analysis for these theories, however, will be easily developed by those interested, and need not be here indicated *in extenso*.

Recapitulation of the Analysis

The deductive applications of the analysis already presented should serve to establish (so far as that is now possible) the principal contentions of the present chapter. These contentions may be briefly recapitulated at this point. (1) Our analysis of animal behavior with reference to objects remote, in space and time, from the organism yielded a proof that causal factors of a non-physical type—that is, mental or psychical factors—are involved in such behavior. (2) This demonstration, if conclusive, refutes all doctrines of the mental which define it (a) as a series paralleling the concomitant physical series but without exerting any influence on the latter; or (b) as one aspect of a double-aspect entity or series, of which physical concomitants are the other aspect; or (c) as an emergent or quality of physico-chemical processes, and interpretable, without residue, in terms of the latter and their organization; or (d) as one or more types of matter or energy, actually, or by hypothesis, embodied in the organism; or (e) as a special type of relationships between physical objects; or (f) as some or all of the reactions of the body to its environment, whether with or without consciousness as an epiphenomenon; or, finally, (g) as contexts or manifolds of neutral entities which, in other contexts, constitute physical objects.

These conclusions rested (1) on the assumption that there *are* physical objects and processes, or, in terms of our neutral conception, sensory elements, including their complexes and sequences; (2) on the discovery that physical objects and processes as reported by the physical sciences—and with the most generous allowance for gaps

[1] The dimensional theory of consciousness was advanced by Pitkin in *The New Realism*, pp. 434–467, and further developed by Spaulding, *The New Rationalism*, pp. 470–486. Pitkin's doctrine has many points in common with the relational theory of consciousness, while Spaulding's version of the doctrine, though admitting its affinities with the relational theory, is essentially the same as that offered or implied by organismic conceptions of the organism.

in our knowledge of them—do not completely account for the behavior of an animal organism with reference to objects and events remote from the organism in space and time; and (3) on the inference from these propositions, that factors of a non-physical, or psychical, type must therefore be involved in animal behavior. From this demonstration there followed, as corollaries, the conclusions adverse to rival theories of the psychical, as recapitulated above.

Correlation of the Results of the Analysis

Our main conclusion, with its implications respecting other theories, is supported in striking fashion by the comparative analysis of animal organisms and inanimate bodies, presented in the first part of this chapter. That analysis, be it noted, is entirely independent of the analysis upon which is based the conclusion that animal behavior involves a non-physical type of causality. It was shown by this comparative analysis, that organisms (particularly human organisms) have as characteristics or components (which are *not* also attributes of inanimate bodies), sensory elements of the organic and motor groups, emotions, feelings, sentiments, ideas, images, memories, thought processes, together with more complex derivatives of these entities, such as desires, interests, volitions, and knowledge in its various stages and forms.

Now, it is just these properties of the organism which bridge the gap between it and objects remote from it in space and time, toward which its behavior is directed. More precisely, it is the non-sensational elements of this whole complex that serve to constitute a bond between the organism and those distant objects or events, since the organism's sensations are, and could function, only where the organism is situated, and there played upon by sensory stimuli from the immediate environment. Sensations could not in themselves, therefore, be component elements of the bond between the organism and the distant objects with reference to which it acts.

More complex mental states or processes, *so far as these are composed of sensations,* are by the same reasoning ruled out as constituent elements of such a bond. Organic or motor or both these kinds of sensations are now generally regarded as the dominating components of feelings, emotions and conations (expressive states), and are claimed by some psychologists to be important components of volitions and thought processes; while so-called external sensations are of course component elements of our perceptions. Sentiments,

desires, interests, ideals, complex cognitive processes, being syntheses of mental states included in this inventory, must all have their sensational elements. But all these mental states, without exception, also embrace as components, ideational elements, or else they are very intimately correlated with such elements. Moreover, such complex syntheses of sensational and ideational elements imply the presence of active organizing processes (these of course correlated with the central nervous system), which are not, for the given organism, sensational elements. Since, however, these organizatory factors are dealt with elsewhere in this volume, we may confine attention here to other non-sensational elements or factors in complex mental states.

We note, lest failure to do so might furnish an occasion for criticism, that the organism's sensory processes are integrated not only with its other mental processes, but also with its physiological processes, whether or no these latter be identifiably correlated with the organism's mental processes. We also note—and for this we must make explicit allowances—that many of the ideational elements in complex mental processes are derived, in part, from sensations. We refer to memories of past objects or events, anticipatory images of future objects or events, and class concepts, inferences, judgments, beliefs, etc., which involve images or other elements partially derived from past sensations.

Now, the ideational elements in these processes which derive in part from past sensations are at once less and more than those sensations themselves; less, in not having the intensity and definiteness of the latter, their relevance for immediate adjustments of the organism to the environment, or their primary value for knowledge; more, in being the elements or instruments whereby the complex structure of knowledge, and the manifold desires, purposes and interests correlated therewith are developed. Moreover, the sequences or organizations of sensations and of ideational elements derived therefrom are very different. Other differences could be pointed out, but those noted serve to show that ideational elements derived in part from sensations must also embrace components of a non-sensational sort. So much, we suppose, is generally admitted, and is indeed demonstrated by a comparison of the factors or conditions respectively involved in sensations, and in ideational elements derived, in part, from sensations.

We note, in addition, that inference, meaning, judgment, the genesis of class concepts and other cognitive processes embrace elements which are neither sensations nor ideational elements derived therefrom, since

those processes at least imply types of relations between sensations or sense derivatives which are not themselves sensations, images or other derivatives of sensation.

Now, some very obvious empirical facts provide an additional demonstration that these various complex states, or a number of them, include non-sensational elements; and at the same time a demonstration that the latter, but not sensational elements as such, constitute the bond between the organism and the distant objects toward which its behavior is directed. Take again a distant city toward which I direct my movements. I do not, until I arrive there, have any perceptions of objects or events in that locality. I do not see, hear, smell, taste or touch anything there; or have organic or motor sensations induced by stimuli thence emanating.[1] But I *desire* sensations and derivatives thereof which would come from being there; I have *emotions* and *sentiments* pertaining to persons or other objects there; I *will* to do or attempt certain things after arriving there; I have *interests* and *purposes* related to that city; I *know* or *believe* that certain objects are there, and that certain sorts of experiences will come of my being there; I know or have a belief as to the means of travelling there; I *infer* or *judge* from evidence that certain events have transpired or will transpire there; a letter, telegram, newspaper or other physical phenomenon *means* (to me) certain objects and events in that city, and so on.

Not sensing or perceiving anything in the distant city, all these other processes, which do refer to that city, must embrace non-sensational elements. It should be equally clear that these other processes taken together are what determines my journey to that city (though not the specific movements of my body or of other bodies that transport me there), and guides my movements toward that city, as well as in it after I arrive there. Leave out any great portion of these complex mental processes, and this type of behavior would not occur. And, as we have shown, this instance identifies the generic type of animal behavior, varying though it does in complexity and range of spatio-temporal reference.

We need not stop to consider whether the distinctions between mental processes indicated by this description do not give a somewhat redundant inventory of our mental outfit. At least cognitive processes and affective states of the organism are essential components of the bond here under consideration. And with the sensational com-

[1] Sounds coming by way of telephone or radio are of course an exception, but these do not invalidate the argument, since action with reference to distant objects does not necessarily depend on such sensations.

ponents of the affective states eliminated, it seems not unlikely that we should have remaining only the cognitive processes, if we leave aside organizatory factors of other types that may be involved in the synthesis of affective states, and here omitted from consideration. Apparently it is meaning, inference and other cognitive processes that, so to speak, light up our affectional states and give them their wide range of spatio-temporal references.

The main point toward which we have been driving is this: It was found by the prior analysis that the definitely directed movements of an animal organism toward a distant goal are not completely accounted for by the matter and energy involved in those movements, and that therefore causal factors of a non-physical type must be active therein. Recall, now, that matter and energy, or physical objects and their motions, are either constituted or indicated by the sensations of the organism—more particularly, visual and other external sensations. Indeed we adopted the term "sensory elements" as the most appropriate designation for a conception of matter and energy, which should be neutral toward the various epistemological conceptions of these entities, and the relation thereof to sensations. A point not made explicit before is that matter and energy are not, on any type of theory, identified with or extended to non-sensational elements of our experience—except, as in mechanistic exegeses, and, as it were, retroactively, to degrade those non-sensational elements to the status of epiphenomena, aspects, emergent qualities, etc., of matter and energy. The physicist and chemist certainly do not, in their laboratories, pay any attention to other than the sensory qualities, with the groupings and displacements thereof, identified with the material they investigate.

Matter and energy are, then, identified with or deduced from sensations, in one way or another. And we found that physical factors, thus come by, do not account for animal action with reference to distant objects. Now we find, by looking at the matter from another standpoint, that substantially the same conclusion emerges. We see that it is not sensations but non-sensational elements of the mind which constitute just the bond between the organism and the distant termini of its behavior, that matter and energy (or our neutral sensory elements) do not account for.

It may be said, by way of depreciative criticism, that we have been dealing in the two lines of analysis here compared with much the same facts and that, granting the validity of the analysis in both cases, they yield not two, but only one, proof of the conclusion to be established. We would say, in reply, that, granting the premise of this contention, its conclusion by no means follows therefrom. *If* it

is the same series of facts, these are treated from two different standpoints and yield two different proofs (if that may be claimed for them) of the same conclusion. The different standpoints with their corresponding types of analysis are those of physics and psychology (not behaviorism), respectively. Our contention here is that the identity of the results yielded by two very different types of analysis must be significant, and supply some additional testimony at least for the validity of those results. If the facts considered in the two analyses are only partially the same—as we ourselves should hold—we have two proofs yielded by different types of analyses applied to partially different series of facts, wherefore the independence of the two proofs is even less debatable.

This completes our present analysis of mechanistic and other conceptions of the mental which offer some sort of physicochemical interpretation thereof, or deny its influence on the behavior of the organism, or impugn the validity of any ontological distinction between the mental and the physical. In the ensuing chapters analyses of typical conceptions from this group are undertaken from other points of view, the results of which, as we shall see, lend additional support to the conclusions yielded by the foregoing discussion. In the next chapter, devoted to a systematic analysis of factors in the activity of the organism, particularly as represented by human experience, it is necessary to examine, with a view to evaluating, theories of the mental not considered in the present chapter. It is hoped that the discussion there undertaken will also throw some light on the questions at issue between different epistemological conceptions of the physical world, and prepare the way for a consistent theory of psychophysical relationships.

The Physicochemical Conception of Physiological Processes, Logically Characterized

One additional topic must be considered, though very briefly, in this chapter. It was pointed out earlier in the chapter that mechanistic theories of life, logically considered, represent a type of deductive rationalism, which is carried through by a hypostasization of certain features of reality into *the* reality, or the fundamental reality, and the interpretation or definition of other features of reality in terms of the hypostasized features. It was also pointed out—what of course is well known—that other conceptions of reality take as fundamental or primary just those features of reality which are given a subordinate position in the mechanistic conceptions; those conceptions, in

turn, being carried through by definition and interpretation, in much the same way as are the mechanistic conceptions. The doctrines cited as illustrating these antithetical rationalisms build respectively from mind and matter, or the psychical and the physical, giving of course the types of metaphysical monism designated as idealism and materialism.

A similar procedure has been employed in the interpretation of the organism on its purely physiological side. We have, again, the monistic materialistic interpretation of physiological processes; and, opposed to it, conceptions which make either the organism as a whole or certain assumed factors therein primary to physicochemical processes of the organism. The former type of conception is advocated by various writers already cited in this chapter, while the latter type of conception is well illustrated by Bergson's vital impetus and Haldane's organismic conception of life (the latter not held in a physicochemical nor yet in a vitalistic sense). And it is significant that, just as the mechanists apply conceptions elaborated by investigations of the inorganic, in an interpretation of the organic, so both Bergson and Haldane apply their conceptions of the organic, in an interpretation of the inorganic; Bergson holding that matter is the product of a movement, the inverse of that which is life;[1] while Haldane claims that "science must ultimately aim at gradually interpreting the physical world of matter and energy in terms of the biological conception of organism."[2]

We cite these antithetical conceptions not for the purpose of attempting here any evaluation or adequate exposition of them, but as showing the possibility of interpreting life in two opposite senses, and by much the same type of procedure, that of hypostasizing certain features of vital processes and subordinating all other features to these. Nor are we interested here in criticising the procedure involved in the elaboration of these conceptions. Our interest is in showing that neither type of theory necessarily occupies a logically privileged position: both are held by able thinkers, and weighty evidence in support of each may be adduced. Evidently both are equally *sub judice* at the present time; and the relevant evidence, properly evaluated, must decide between them. One of them may turn out to be correct, in the main; or it may be that the truth falls somewhere in between them, or in a different region altogether. Our own evaluation of the two types of theories, as applied to the physiology of the organism, is re-

[1] One of the central themes in his *Creative Evolution*. See especially pp. 245–250; additional references in the Index under the caption, "Inert matter and action, the inversion or interruption of life."

[2] *Mechanism of Life and Personality*, pp. 98–99.

served for later chapters. Here we have indicated only a very general logical view of them.

Before leaving the problem, however, we should like to consider it in relation to the results yielded by our analysis of sensory experience in the first part of this chapter. It was there shown that current conceptions of matter and energy, of physical objects and events, or of our neutral sensory elements (different designations for the same class of entities) are all theoretical interpretations of visual, auditory and other *external* sensations; and that they have no place, therefore, for the entities or elements constituted or indicated by motor and organic sensations. On the basis of this evidence we concluded (1) that, since all classes of sensations furnish data for a theory of the physical world, and different types of sensations are not reducible to terms of one another, the current conceptions of the physical world are necessarily incomplete, being based only on external sensations; and (2) that sensory elements, matter and energy, or physical objects and events constituted or indicated by motor and organic sensations, are peculiar to organisms alone, not to inanimate bodies, since these groups of sensations can in nowise be referred to bodies of the latter class.

Now, this body of evidence bears strongly against any pyhsicochemical conception of the organism on its purely physiological side, and, so far as it goes, establishes the proposition that organizatory factors irreducible to physicochemical terms are at work in the organism. For the sources of the *stimuli* to the motor and organic sensations are *organized* structures and functional activities of the body, as a reference to our inventory of these sensations will clearly show.[1] That these structures and activities are organizations of physicochemical processes is of course a plain fact; but these processes, taken without the organization, would be or would stimulate only the so-called external sensations, not the motor or organic. And no mere compounding of those processes would yield stimuli or sensations totally different from those which they themselves are or produce. There is, so far as we can see, no *tertium quid* over and above these processes and their organization, whence the conclusion seems inescapable that the organization is or stands for a factor distinct from the physicochemical processes correlated with it.[2]

[1] See pp. 190–194.

[2] It is not here implied that mental functions do not have anything to do with the development, maintenance and activity of the structures whence emanate the stimuli to motor and organic sensations. In order not to complicate the analysis, such possible mental functions are, by implication, subsumed under the organizatory factors explicitly treated in this connection. The relationships between mental

Our evaluation, in this chapter, of physicochemical conceptions of the organism, *via* an analysis of our sensory experience, has necessarily been presented in a rather awkward way, owing to our plan of conducting it on a neutral basis, as regards divergent epistemological conceptions of the physical world and its relation to sensory experience. And we have ignored problems and contradictions involved in these conceptions, or suggested by our own neutral attitude toward them. For example, all sensations seem to depend on organisms so constituted as to be capable of experiencing sensations, and this raises the question whether sensations can be wholly accounted for in terms of physical objects and events as such, since certain of the objects and events involved in sensation appear to be differentiated from the others by some additional determinations. This problem we could not recognize, because we wished to test physical conceptions of the organism on their own ground, and these rest on certain basic assumptions as to that problem which could not, on our method, be called in question. To this and other problems suppressed in the present chapter we now turn.

functions and organizatory factors not clearly identifiable therewith are discussed elsewhere in the text. Note that two somewhat different conclusions are supported by our analysis of motor and organic sensations: (1) that organizatory factors, distinct from physicochemical process, are involved in such sensations; (2) that features or elements of the physical world are indicated by these sensations, for which current conceptions of the physical have no place. *Both* these conclusions seem to us well grounded, and not to be incompatible with one another. But they are deserving of further investigation.

CHAPTER VIII

A FACTORIAL ANALYSIS OF THE PHYSICAL AND THE MENTAL

THE human organism comes somehow by types of experience called sensory. This experience depends in some way on the specific structures of the organism itself, including sense-receptors, sensory neurons, central nervous system and effector organs, together with the functional activities made possible by these various structures. Mind or consciousness is also involved, somehow, in sensory experience—just how is of course a topic of vigorous controversy at the present time. All these conditions of sensory experience are parts, properties or attributes of the percipient organism.

Sensory experience is generally conceded to depend also on events partially external to the organism, but, so to speak, impinging on it and inducing various specific sensations of which it is capable. These events or, in the language of psychology, these sensory stimuli emanate from things outside the organism which we term physical objects. (Certain sensory stimuli, however, come from within the organism.) Many classes of these stimuli are transmitted through a medium of some sort (it may be empty space in certain cases) before they reach the several receptors that are sensitive to them; and all these stimuli, or derivatives thereof, must be transmitted along nerve-fibres to the brain before the several specific sensations induced by them can occur. These various sensations are of course related in manifold ways to phases of our experience which are not altogether sensory in character, as imagery, sentiment, reflective thought, etc.

This very general inventory of the factors or conditions of sensory experience would be accepted as valid, we suppose, by all schools of philosophical thought. But most divergent theoretical constructions are put upon it. Idealists hold that the sensory qualities given in our experience are altogether mental in composition. Neo-realists hold that they are part and parcel of the physical object, but that they may be directly presented to or included in the mind. Radical empiricists hold that they are neutral components of both minds and physical objects. Critical realists hold that sensory qualities are complex resultants of interactions between minds and physical objects, but that through these qualities physical objects can be known or affirmed.

Both minds and physical objects are accepted as in some sense real by all these conceptions, and as involved, somehow, in our sensory experience. The differences between these conceptions relate to the rôles played in sensory experience by physical and mental entities respectively; and (what is much the same thing) to the line of distinction, the frontier, so to speak, between the mental and the physical. The idealist puts the frontier far over into the territory of what the realist regards as physical, and indeed implicitly absorbs all the physical into the mental, though incorporating as a sort of surd in his system factors in sensation which are not mental (at least in the more common sense of that term). The realist, in turn, puts the frontier over into the territory of what to the idealist is mental, though not necessarily going as far in that direction as does the idealist in the opposite direction. The radical empiricist grants the demands of both idealist and realist, so far as sensory experience is concerned, and begs them to desist from their dispute as an altogether idle one. Sensory qualities, says the radical empiricist, are physical or mental or neither, depending on how you take them. The critical realist for his part takes this problem seriously and makes an ostensibly more equable division of the sensory field between the physical and the mental.

A solution of the problem thus defined seems well-nigh impossible, because the mental and the physical, or what various parties to the controversy would regard as both of these, are always conjoined in our experience, and it is impossible to find or produce experimentally a case in which only the one or the other would by common consent be present. Therefore, both in the construction and the criticism of these several theories, the mental or the physical may be emphasized, or both regarded as of somewhat equal significance, or, again, viewed as neutral to the whole sensory business.

We propose to present a very brief critique of these doctrines, especially as regards their interpretation of sensory experience, and formulate, on the basis of this criticism, our own theory of the relationships between the mental and the physical. It will be convenient to give first a brief statement of our theory, and elaborate it concurrently with the discussion of the theories opposed to it.[1]

[1] We recognize, of course, that it is not satisfactory to deal with such weighty matters in so brief a space as we are here limited to. We do not for that reason propose to do more than indicate the grounds upon which the doctrines opposed to ours are rejected; and present a very provisional statement of our own doctrine and the grounds upon which it is based. We do not profess to deal at all adequately with the evidence adduced in support of these various doctrines, or with all the objections that might be raised against our own doctrine. The whole discussion

The Sub-Physical and the Sub-Mental

The way out of the impasse just described, it seems to us, is to go back of the mental, the physical, and their combinations, and propound some new categories whereby to account for these ubiquitous combinations, and the continuous series thereof falling within the limits—never reached (indubitably) by the combinations themselves—set by the purely physical and the purely mental. We propose as two such categories entities which we shall designate as the *sub-physical* and the *sub-mental* respectively. The entities for which these terms stand are conceived through an *analysis in situ* of the physical and the mental, with their combinations.

According to our theory, no entity of either class is ever completely isolated in experience from some entity or entities of the other class, and is therefore never *directly* experienced as such. What we know as the mental or the physical is always the product, in some sense, of interactions between the sub-mental and the sub-physical. The mental as ordinarily conceived stands nearer the sub-mental than the sub-physical, but never reaches it as a limit (in our experience); and the physical as ordinarily conceived stands nearer the sub-physical than the sub-mental, but likewise never reaches it as a limit, in experience. But the sub-physical may and does to a large extent exist independently of the sub-mental, and there is some evidence, though not of a wholly convincing character, that the sub-mental may exist independently of the sub-physical. Nature as we know it is the resultant of interactions between the sub-physical and the sub-mental (together with factors of other categories to be discussed later), and cannot be consistently conceived in exclusive terms of either.

The hypothesis thus briefly sketched lies somewhere in or near the region occupied by the critical realists, though the particular ontological doctrines we are advancing would perhaps not be accepted without substantial qualifications, by any accredited member of this school.

must obviously be presented, for these reasons, in a rather dogmatic way. If the philosophers will let our infant doctrine live, we promise to take it up again on a later occasion, investigate it more thoroughly, and, if deemed deserving, give it such further elaboration as we may be capable of. Their coöperation in this undertaking would be cordially welcomed. It need scarcely be added that some treatment of the problem here indicated is called for by the general plan of our inquiry, since one of our chief aims is to examine critically mechanistic and other depreciative conceptions of the mental, and at the same time formulate in general terms a consistent conception of what may properly be termed physical. This will obviously be a continuation, by a different method, of the discussion of the relations between the mental and physical presented in the preceding chapter; and, more particularly, of the problems raised by different epistemological conceptions of the physical there indicated.

Our position is of course opposed to the idealistic and neo-realistic doctrines on the same problem, and must be justified by a criticism of these doctrines, together with such positive evidence as may be indicated by this criticism and the more detailed elaborations of our position on its own account. This discussion will carry critical implications as to the position occupied by the radical empiricists; and this fact, together with the affinities between our position and that of the critical realists, will enable us to concentrate the more controversial part of our analysis on the doctrines held by idealists and neo-realists respectively. We shall deal first with the neo-realistic position in this connection.

Neo-Realistic Conceptions of Sensory Experience

There are at least two different types of neo-realistic theory as to the nature of sensory experience. One of these maintains that a color or other sensory quality is "out there" on the particular object where it appears to be, and that it does not derive any of its quality from the medium whereby it comes within the range of our experience, or from the physiological processes of the organism, or, again, from the mind or consciousness experiencing the quality. Sensory qualities are alleged to be presented to or included in consciousness as they objectively exist in their own right.[1] A second type of neo-realistic theory maintains that the sensory quality derives its nature from the particular object where it appears to be and from the physiological processes of the organism and also, in some cases, from the medium transmitting the stimulus, but not from psychical entities distinct from these various physical factors.[2] Advocates of either theory differ among themselves, of course, in the detailed elaborations given the particular theory, but these differences we need not consider at present.

Both these types of neo-realism agree that sensory qualities do not derive any of their nature from distinct psychical entities; and a variant of one type even holds that the mind is only a dimension or synthesis, though of a qualitatively distinct type, taken on by chemical processes and other physical components of sensations, together, perhaps, with non-physical entities not entering into sensations but not in themselves properly termed mental (Spaulding, Pitkin). The differences between the two types of theory, on the other hand, pertain

[1] The view represented by Alexander, S., *Space, Time and Deity, passim,* esp. Vol. I, pp. 269–278, and Vol. II, pp. 52–59, 138–142; and by Holt, E. B., "The Place of Illusory Experience in a Realistic World," in *The New Realism* by Marvin, Perry, Spaulding, Montague, Holt, and Pitkin, pp. 308–355.

[2] The view of Bertrand Russell, *Our Knowledge of the External World,* Lecture III; and Spaulding, E. G., *The New Rationalism,* pp. 476 ff.

to the sorts and amounts of entities not mental which contribute to the nature of sensory qualities.

Before discussing separately these two types of neo-realistic theory, we may offer some comments that apply to them both. It may be pointed out, first, that arguments of the type presented in support of these theories can be used and have been adduced in behalf of the opposing theory, namely, that sensory qualities are altogether mental in character. For, what *both* the realistic and the idealistic theories do is to select *certain* of the apparently distinguishable groups of conditions involved in the experience of sensory qualities, as accounting for the latter, and exclude all other conditions as contributing nothing to the nature of these qualities. Of course either one of the two opposed types of theories is intelligible and might conceivably be proved correct, but so far neither of them has been able to show with any conclusiveness that the conditions appealed to by its opponents are not prime factors in the production of sensory qualities, and because *all* these conditions are found to be present whenever sensory experience occurs. More precisely, mental processes of the organism and various physical conditions which appear to be partially distinct therefrom are always involved in sensations, and neither of these has proved to be reducible to terms of the other.

These statements of course carry implications which are disputed by the two parties to the controversy, and must therefore be considered further. The point here is that, so far, neither party has been able to carry through its program of excluding as prime factors in sensory qualities the conditions appealed to by its opponents, and *prima facie* involved therein. This point will be significant for the ensuing discussion, since it justifies us in assuming that *all* the empirically distinguishable conditions essential to the experience of sensory qualities have a logical claim to be prime factors therein, unless conclusive evidence can be produced that such is not the case. The burden of proof appears to rest on the several parties to the controversy who contend that this is not the case.

We may observe, secondly, that the neo-realistic position respecting sensory qualities appears to be largely maintained in the interests of a certain epistemological theory, namely, the theory of external relations, which represents a reaction against the theory of internal relations that underlies the dominant types of contemporary idealism. While some realists admit that many sorts of relations may be of the modification and underlying-reality types, all realists tend to emphasize the relations deemed to be external and often talk as if all relations were of this character. There is no pronounced tendency among them

to approach the detailed investigation of relations in an empirical way, and without bringing to such investigation any presuppositions as to the types of relationships which might be disclosed thereby.

Into these opposed theories of relations we cannot here enter, but must content ourselves with a very brief comment on the matter. The modification type of relationships is evident throughout the domain of social life, for the development of the individual or of the social group cannot be accounted for, as sociologists recognize, without assuming that diverse types of factors interact to determine all such development and, in interacting, modify one another—giving complex syntheses, of which the primary factors in a pure form cannot any longer be identified. Moreover, our discussion, in the first part of this book, of the genesis of hereditary variations goes to show that the same type of relationships prevails in this most important field. Now, if the modification type of relations is demonstrable in these fields, it is quite possible that it may be found elsewhere. Is it to be found in the field of sensory experience? The question—and that is the point made here—must not be prejudiced by any *general* theory of relations, but investigated on the basis of the data relevant to it. Let us see how the neo-realistic theory deals with these data, considering first that sub-type of the theory which attributes sensory qualities to the particular objects "on which" they appear to be.

Holt concludes, after a survey of evidence from nerve physiology, that secondary qualities cannot be accounted for by specific energies of the sensory nerves, or by specific differences in cortical substances involved in sensation, or by peculiarities of the synapses also involved in sensations. His own theory is based on evidence tending to show that there are periodic fluctuations in the sensory nervous impulses corresponding in number and rhythm to similar features of the stimulus. This, together with the fact that light, heat and Hertzian waves belong to the same physical order, furnishes a basis for the hypothesis that all sensory qualities are reducible to elements of one sort, organized in manifold ways corresponding to the various sensory qualities. Where the component elements of the stimulus and the corresponding nervous impulses have a very high frequency rate, as in visual and most other sensations, the time-sense cannot discriminate them. What we do perceive is the frequency magnitude or density of these impulses. Density is said to be perceived "for very high frequencies by a process which is perhaps related to physiological summation," and the secondary qualities to be "form qualities of which the time-sense is inadequate to perceiving the form."

Holt does not appear to present a consistent conception of the

element (referred to in some passages as a primitive sensation) alleged by him to be the fundamental component of all sensory qualities. This is regarded in different passages as in the nature of unspecialized nervous shock, and as electromagnetic vibrations from the external object.[1] Perhaps these are regarded as qualitatively identical. In any case, since Holt always insists that the sensory quality is on or part of the object, we may identify his relatively primitive sensations with what in psychology is spoken of as the component elements of the stimulus. It is perhaps important to note that Holt does not regard the density factor of the *nerve impulse* as the secondary quality.[2]

Alexander's view is similar in a general way to Holt's and a brief reference to it must suffice for our present purpose. Quality belongs, he holds, to the motions of the spatio-temporal thing or substance and, conversely, substance is regarded as a specially defined volume of space-time stippled over with qualities.[3] But the secondary qualities, like qualities on other emergent levels, such as life and mind, are motions of various orders of complexity.[4] Neither mind nor physiological processes contribute, therefore, to the nature of the secondary qualities. The mind and the neural process (identified as one by Alexander) only sense or enjoy the qualities.[5]

Without expounding further this type of the neo-realistic theory, let us consider whether it exhibits sufficient grounds for the reference of sensory qualities exclusively to the objects on or of which they appear to be. Formulated in relation to a point made previously, this question will be whether adequate grounds are offered us for rejecting the view that external object, intervening medium, physiological process and mind or consciousness all contribute to the nature of sensory qualities; for all of these, as we saw, have *prima facie* equal claims to be regarded as rendering such contributions.

To economize space, our answer to this question must be presented very concisely and in a somewhat dogmatic fashion.

We will say first that, so far as we can see, what is *proved* to reach the organism from the external object is by no means the same as, nor even very similar to, the sensory qualities in which, as we fully concede, the external object is a prime factor. The electromagnetic vibrations involved in visual sensations have no resemblance to brightness or color; nor do air waves have any resemblance to sounds. It is claimed by Holt that the secondary qualities are *form* qualities of

[1] *Op. cit.*, pp. 326, 350–352.
[2] *Ibid.*, p. 352.
[3] *Op. cit.*, Vol. I pp. 270–276.
[4] *Ibid.*, Vol. II pp. 52–59.
[5] Vol. II, pp. 138–142.

the qualitatively identical components of all sensory qualities, and that the different densities of these elements are perceived by a process related to physiological summation. But without this process there would presumably be no sensory qualities or at least no perception of any such qualities. And nothing that Holt says about the matter appears to exclude the possibility that the organism contributes to the nature of these qualities, as well as to the experience of them.

We can go further and say that air waves or light vibrations, as physics reports these to us, are not endowed with form qualities which, according to Holt's conception, are the distinguishing feature of the secondary qualities. These elemental components of the stimulus come directly one after the other, in regular succession, and do not as such give sensory qualities. It is doubtless true that the empirical differences between them and the corresponding sensory qualities are partially due to differences in the processes of recording or estimating the two series. In some cases, indeed, different senses are employed in the registration of the stimulus and its correlative quality. But, to repeat, no grounds have yet been offered us for the complete identification of qualities with the stimuli from the external object. Until that is done we seem justified in holding to the *prima facie* view that other conditions involved in the experience of sensory qualities are prime factors in the latter, equally with the stimuli which reach the organism from the external object.

Some additional remarks may be offered on the view that sensory qualities are on or part of the external object. Consider, first, the visual qualities. If physics has demonstrated anything in this field, it is that the external factors in visual sensations are electromagnetic vibrations falling within certain limits of frequency or, in more general terms, disturbances of some sort emanating from self-luminous objects. These vibrations or disturbances impinge on all the objects not far removed or otherwise isolated from them, and are reflected in part to other objects, including organisms sensitive to such vibrations. How, then, can one maintain that an object possesses color in its own right? It reflects something not its own, something that is entirely accidental to it. The color we associate with an object, curiously enough, is correlated with the classes of light vibrations which the object *rejects,* the vibrations which it is unable to incorporate in itself. Perhaps the point is a trivial one, but we should say that if visual qualities can properly be ascribed to objects at all, such qualities would be precisely those we do *not* associate with the given objects, unless indeed the light vibrations rejected (reflected) by an object are a part of it, but not those accepted (absorbed) by it. We should not for

our part attribute color or brightness sensations to the external object at all, but, instead, molecular or other properties by which it absorbs certain classes of electromagnetic vibrations and reflects other classes of these vibrations.

Rather similar observations hold for sounds and their relation to external objects. How can one assert, as does Alexander, that "it is the bell which sounds, not the air between the bell and our ears"? [1] We apprehend sounds, whatever their nature, by means of something that reaches our ear and possibly our nervous system from the outside. This something we call air waves. The bronze of the bell and its vibrations do not reach our ear or nervous system; if they were to do so, it is certain that sensations of any sort would promptly cease. Without air or other substance between ourselves and the vibrating object sounds would not be produced at all, as has of course been experimentally proved by observing vibrating objects in a vacuum, whence nothing that could be called sound issued forth. Incidentally, sounds in which vibrating objects constitute one factor derive *some* part of their qualities from the medium, as is shown by a comparison of sounds involving the same vibrating objects but different media between the latter and the percipient organism. The contribution of the object to sound therefore comes from its shape, size, physicochemical composition and possibly other characteristics involved in its particular sorts of vibrations.

All these facts appear to justify the conclusion that none of the events which truly "belong" to the external object, nor any of the events in the medium between the object and the organism, are equivalent to the visual and auditory qualities of which they are factors.

We cannot be so explicit with regard to touch, taste and odor sensations, and because these have not been investigated so thoroughly as have visual and auditory sensations. One difficulty here, as in the case of visual and auditory sensations, is that stimulus and sensory quality are investigated in part by means of different senses and, even if they had identical elements in common, we should find it hard to determine just what they were. If we *could* keep to one class of sensations alone, we should be able to say little more than that these sensations depended in some way on influences reaching us from the outside. We can say, however, with the aid of other sensations, that the external factors involved in taste and odor, and perhaps in touch sensations as well, come nearer being *parts* of the objects to which those sensations refer, than is the case with the external factors in visual and auditory sensations. For in odor and taste sen-

[1] *Op. cit.*, Vol. II, p. 57.

sations at least we seem to be reacting to real *pieces* of the given objects in a way not characteristic of visual and auditory sensations.

Certain evidence of an inferential sort—but not on that account less valuable—strongly indicates that the olfactory, gustatory and tactile qualities derive part of their nature from the characteristics of the organism experiencing them. Not only are there different sense qualities correlated, in the case of the same individual at different times, with the same sorts of stimuli, but different individuals also appear to differ widely in their sensations from the same sorts of stimuli, while such differences between different groups of animals are indicated by their different responses to the same sorts of gustatory, olfactory and tactile stimuli.

The first two types of differences in sensory qualities correlated with the same sorts of stimuli, namely, differences between individuals of the same species, and between the experiences of the same individual at different times, would be partially, perhaps altogether, due to associational and other factors influencing the given class of sensations, although we are inclined to think that not all such differences could be thus explained. Even if they could be, we should still have left the differences between different groups of animals, which could not be accounted for in this way, since they are largely due to hereditary peculiarities, as the differences between carnivora and herbivora, for example, clearly demonstrate. This evidence appears to contravene the type of neo-realistic theory here under consideration, showing, as it does, that the same sorts of external stimuli induce, in different organisms, sensory qualities which are not of the same sorts.

The organic and motor sensations have been investigated far less, relatively, than any other groups of sensations, and indeed but little has been done in the way of identifying the specific sorts of stimuli involved in these sensations. And here, as in the case of the so-called external sensations, sensory quality and stimulus are described in terms of data yielded by different senses. Another difficulty is that the stimuli to the motor and organic sensations must differ somewhat from individual to individual, proceeding as they do from component structures and functional activities of the organism, which are not of precisely the same sorts in different individuals. But stimulus and sensation appear to be very different here, and we can think of no grounds upon which their identification could be justified. Unless such grounds shall be pointed out, therefore, we seem justified in holding to the view supported by the empirical facts taken at their face value, namely, that physiological changes identifiable as the stim-

uli, sensory nerve process, central nervous system, and mind or consciousness all contribute to the nature of these sensory qualities.

It would appear, moreover, that insuperable difficulties beset the present type of neo-realistic theory in its attempt to reconcile the claim that secondary qualities are part of the external object, with the fact that the organism, which is separated from the object, experiences or senses the secondary quality. It is admitted that this sensing depends on something from the object reaching the organism; and yet it is claimed that the secondary quality does not get into the space between the object and the organism, nor yet in the nervous system of the organism, nor again in a mind or consciousness located in the same space as the organism.

The reference here is more particularly to Holt's theory of the secondary qualities. Thus, in one place he says that "we are little tempted to believe that the color of a flower fills all the space between the flower and the eye: and neither less nor more does it fill, or enter into, the peripheral nerves and skull. The entity responded to (he continues) is the color out there, two factors which involve two factors of response; but *that color out there* is the thing in consciousness selected for such inclusion by the nervous system's specific response." [1] But elsewhere in the same essay he says "that the facts of nerve physiology point unmistakably to the view that *the quality of sensations is transmitted to the brain by vibratory nerve-impulses*" [2]; while he later warns us that he has not said "that the density factor of the nervous impulse is the secondary quality," but that "it is the density of the series of some relatively primitive sensation which *is* the secondary quality." [3]

The contradictions in these statements, when brought together, are so patent that it is scarcely necessary to point them out in detail. It is asserted that the sensory qualities are in and not in the brain; that the quality of sensations is transmitted by nerve impulses, and that nerve impulses are not the secondary quality; that the quality of sensations reaches the nervous system, and that it is not in the space between the object and the organism. Why does so able a writer fall into such glaring contradictions?

The answer, we should say, is because Holt has set himself an impossible task. He must maintain that full-fledged secondary qualities are "out there" where they appear to be and nowhere else; and, at the same time, in order to account for the organism's response to or

[1] *Op. cit.*, p. 354. Italics in original.
[2] *Ibid.*, p. 325. Italics ours.
[3] *Ibid.*, p. 352

experience of them, he must somehow get them through the intervening space into the nervous system of the organism, and thence back again out there where they appear to be, this "out there" having by this time, and in some mysterious way, been included in the consciousness of the organism, which is not (in this instance) associated with the brain and nervous system of the organism, but is "out there" where its objects appear to be.[1] The proposition to be maintained inevitably leads to contradictions, and there is no way of eliminating these contradictions, so far as we can see, without abandoning the proposition itself. For it we must substitute a procedure which accepts all the empirical facts of the situation as we find them, and weaves these into an internally consistent conception not entailing the distortion of any of these facts or the assumption that any of them are totally different from what expert investigation shows them to be.

What we *know* is that events of some sort occur in or on the object, that these or dissimilar events induced by them in a medium reach the organism, that sensory receptors react to these impinging events in various, more or less definable ways, that impulses, also more or less definable, are thereby induced in the sensory nerves, that these or their derivatives reach the brain, and that from this complex interplay of events sensory qualities are experienced. The latter are unlike any of the events entering into this complex interplay, and there are no compelling reasons for believing that these events do not all contribute to the nature of the secondary qualities, while there is a good deal of positive evidence (partially indicated in the foregoing discussion) that they severally do so contribute.

The place where all the raw materials of the sensory quality are assembled and its manufacture from them occurs—if we may so put it—is where the organism experiencing the quality is situated. By some complex process the quality is then referred—projected if you will—to the object from which proceeds, as experience has shown, the events initiating the whole complex interplay of events involved in sensation. The biological utility of such reference is obvious, and doubtless accounts for the fact that such processes of reference are to be found in the world, though not for their ultimate origin. The fact that the secondary quality often persists (in a derivative form) as an image which, so to speak, belongs to the organism, and after the primary stimulus no longer affects the organism, is additional *prima facie* evidence that the events which give rise to the quality are focussed, not on the object, but in the organism.

[1] As to the latter point see *op. cit.*, pp. 353–354, and the same author's *Concept of Consciousness, passim.*

It is only half the truth to assert, as does Holt,[1] that we act as if the secondary qualities were out there on the objects; for it is equally true that we act as if the secondary qualities were in our organism, seeing that by the movements of our body or of its parts, the secondary qualities coming within the range of our experience are also determined. And if the secondary quality is really located in any definite place—and we should say that this is the case—it must be where the organism has its locus, not out there on the object. For, on our analysis, all the prime factors in the quality never come together anywhere except in the organism, and the quality would therefore be where it is, if anywhere.

Our discussion of the first type of neo-realistic theory has not *shown* (although it assumed) that mind or consciousness (embracing non-physical components) is a prime factor in sensory qualities, because so far we have been concerned only to refute the proposition that the organism and other conditions distinct from the external object do not contribute to the nature of those qualities, and for this it was not necessary to show that components of the organism other than its sense organs and nervous system are prime factors in sensory qualities. It will be necessary to show that factors of a distinct mental order are essential to sensory qualities, if we are to justify our own doctrine that sub-physical and sub-mental entities determine (with other entities) all phases of our experience.

This undertaking should also throw some light on a perplexing feature of the situation thus far defined, the fact, namely, that the physical conditions essential to sensory qualities—properties of the object, events in the medium, physiological processes—are themselves presented to us as groups of sensory qualities, and give rise to qualities very dissimilar to themselves. This fact raises the question—a question answered in very different ways—whether the given sensory quality can be wholly accounted for in terms of the various sensory qualities or of imperceptible entities indicated thereby, which constitute the physical conditions of that quality. The validity of our doctrine depends on the answer to this question also. The fate both of positivism as a metaphysical doctrine and of the second type of neo-realism is bound up with the same question. Our position on this question can best be developed in connection with a critical analysis of the latter doctrine.

This doctrine has been carefully elaborated by Spaulding,[2] especially in relation to sensation, and we shall confine our examination of

[1] *The Concept of Consciousness*, p. 137.
[2] *The New Rationalism*, pp. 470–486.

the doctrine in this application to his account of it. According to his doctrine, consciouness is a class of qualitatively distinct dimensions constituted by a creative synthesis, a non-additive or serial organization, of elements which as such are not conscious. A sensation (or sensory quality) is the whole given by such serial or non-additive organization of non-conscious elements of a certain kind, namely, the entities investigated by such sciences as neurology, physiology, physics and chemistry. The particular sorts of entities specified by Spaulding in this connection are atoms, molecules, colloidal solutions, nerve- and "sense-cells," nerves, ether waves, air waves, masses, chemical valence, electrolytic conductivity and the like, some of which, according to him, are "sensed" while others are not.[1] A sensation is not any such elements taken by themselves, nor the *relations* which subsist between them, but the given elements *and* their (serial) relations.

Such a conscious dimension is said to be extra-spatial and extra-physical, although a dimension of elements which (judging from the illustrations given by Spaulding) *are* spatial and physical.[2] Finally, while insisting that consciousness represents the synthetic creation of new properties, it is argued that these "properties are, not causally, but functionally related to, *i. e.,* correlated with, the (properties of the) constituent parts." [3] Such a relation, says Spaulding, is external. By this is meant that a sensation, for example, is not subjective but objective. It is not completely identified with the various motions entering into the synthesis of which it consists, and is therefore not subjective; but is, on the contrary, a distinct series or dimension of the component series of motions.[4]

Spaulding defends his doctrine against the charge that the entities claimed by it to be synthetized into a sensation are themselves sensations, so that we have sensations organized to give sensations. His defense is that molecules, colloidal solutions, nerves, etc., though related to some sort of *knowing* (specifically, in this case, sensation), are not sense-constituted entities, the conclusion that the latter is the case being "based (1) on the *purely gratuitous postulation* of the modification theory of relations, and (2) on the ignoring of a method of virtual elimination of *knowing* (of any specific form) from the entity that is known." [5]

We may now attempt an evaluation of this doctrine. We should say, by way of recognizing its positive value, that Spaulding con-

[1] *Ibid.,* pp. 476–477.
[2] *Ibid.,* pp. 484–485.
[3] *Ibid.,* pp. 479–480. Italicization in original omitted.
[4] *Ibid.,* p. 480.
[5] *Ibid.,* pp. 476–477, footnote.

tends, correctly, as we think, that *all* the physical conditions involved in sensations contribute to the quality of sensations.[1] That this feature of his doctrine is, to our way of thinking, not only correct but of fundamental importance for a theory of sensation is emphasized by all we have said about the problem up to this point. We are unable, however, to accept Spaulding's assumption that the synthesis, in sensation, of various physical entities is a case of external relations; we hold, on the contrary, that it illustrates the modification type of relations; but as this matter is not of great importance for our present purpose, we need not dwell on it at this point.

Our main criticism of Spaulding's doctrine is that it finds no place for distinct mental (or sub-mental) entities in its account of sensations, that is, for entities other than the physical and their organization. Mind, on the contrary, is alleged to be created from physical entities. In justifying this criticism we should begin by pressing the objection (cited above) against which Spaulding defends his doctrine. For, in our judgment, Spaulding's doctrine is decidedly open to the charge that it explains one sensation (or sensory quality) in terms of other sensations different, in large measure, from the sensation to be explained; or in terms of *non-sensed* entities revealed, both as to their existence and their nature, by those other sensations. We may, for the sake of the argument, accept Spaulding's assumption that the knowing in this situation (more particularly the sensing) can be eliminated without modifying the entities which are known. We can do this because knowing or sensing pertains to the particular sensation to be explained in the same way as to the entities whose synthesis is to do the explaining. If knowing or sensing is to be eliminated anywhere it must be eliminated all along the line.

Then, supposing we adopt some neutral term, such as our former "sensory elements," by which to designate that which remains, the doctrine will be that one sensory element is explained by a number of other sensory elements which are different from the former. But the doctrine would also be applicable, in turn, to the sensory elements whose synthesis explains the given sensory element; and so on indefinitely. Evidently an infinite regress, but one which the doctrine would seem to involve. Moreover, as we follow back this regress, the sensory elements required for explanatory purposes must increase by some sort of geometrical ratio, so that in the end we require an infinite number of sensory elements to account for the one with

[1] We should of course substitute sub-physical entities for physical factors, since, according to our doctrine, the physical represents a synthesis of sub-physical and sub-mental elements.

which we started; and each of this infinite number requires, in turn, an infinite number for its own explanation; and so on.

Let us shift the argument to a more empirical level and, taking the standpoint of the sensation as given, consider whether a color sensation, for example, can be accounted for in terms of the sensations which are, or from which is inferred the existence of, the light waves, properties of the reflecting object, retina, optic nerve, brain and other physical conditions involved in the color sensation. Considering the fact that chemical compounds present sensory qualities very different from those of their component chemical elements, one might conclude that the physical conditions of a color sensation could, through a creative synthesis of themselves alone, give such a sensation, were there no grounds, such as those just indicated, for rejecting the conclusion that this is the case.

But we can say, without appealing to those grounds, that proof of the hypothesis under consideration is wanting, since the arguments advanced in its support have not excluded the possibility that a mind or consciousness not constituted by a synthesis of such non-conscious elements does contribute to the nature of sensory qualities. There is, in this case, nothing approaching the type of demonstration yielded by chemistry that, for example, H_2O induces very different sensations from those induced by hydrogen and oxygen taken separately.[1] And our contention is that the burden of proof rests on those who would claim that mind does not as a distinct type of entity contribute to the nature of sensory qualities. Positive evidence for the existence of mental entities distinct, in character, from physical entities of any sort was adduced in the preceding chapter; and this evidence, together with that adduced in the previous discussion (exposing the infinite regress implied by the opposed doctrine) should suffice to show that such entities are involved in sensory experience; and that these must be regarded, in the absence of conclusive proof to the contrary, as contributing to the nature of sensory qualities.

Recurring to the previous discussion, we may remark that mind or consciousness is the only condition of sensory experience that could eliminate the infinite regress implied by Spaulding's doctrine of sensation, since this doctrine accounts for all the significant conditions of sensory experience other than that. Otherwise stated, our argument shows that Spaulding's doctrine requires for its completion the

[1] This example, which at first sight appears to support the type of doctrine advocated by Spaulding, really incorporates the question at issue, as the organism's mental functions (whatever these may be) are as much involved in sensations associated with water, hydrogen and oxygen as in any other sensations.

assumption that mind or consciousness not constituted by a synthesis of physical entities is a prime factor in sensation, as well as physical entities themselves, with whatever type of synthesis they may be shown by an exhaustive investigation to assume in this connection.

Spaulding's doctrine is also open to criticism in that it makes consciousness an extra-spatial, extra-physical affair, and at the same time a synthesis of elements which, so far as he tells us, are all purely physical and spatial. He gives consciousness this non-physical, non-spatial character in order to account for the fact that it transcends itself spatially and temporally, and refers to objects remote in space and time. He does not tell us how relationships between purely physical and spatial terms (the only sort of terms he recognizes in this connection) can be other than the same sorts of relationships. Spaulding insists, it is true, that "specific organizing relations" are involved in creative syntheses of this type,[1] and that "no realm of fact, whether subsistent or existent, is exempt from this principle of creative synthesis"; [2] but we are left to infer that in the present case these organizing relations "cohere" in purely physical terms, for Spaulding nowhere advises us to the contrary.

Now, to such a doctrine our discussion in the previous chapter applies with destructive force granting its correctness, for we there undertook to show that action of the organism with reference to a distant object involves a type of factor which is not interpretable in physical terms. Moreover, our criticism of the relational theory of consciousness applies to this feature of Spaulding's doctrine of consciousness, since, like that theory, it derives a non-physical type of relations from purely physical terms.[3] So much by way of vindicating the theory that mind or consciousness contains elements distinct from the physical entities with which it may be related, as against the dimensional theory to the contrary; and hence of vindicating the *prima facie* right of such mind (or such elements) to be regarded as a prime factor in sensory qualities.

The Positivistic Theory of Sensation

We have said that the fate of positivism as a metaphysical doctrine is bound up with the answer to the question we have been consider-

[1] See, for example, *op. cit.*, pp. 448, 500.

[2] *Ibid.*, p. 500. Italics in original omitted.

[3] We shall undertake to show in a later chapter that Spaulding's hypothesis of creative synthesis interpreted in a physical sense, as he allows it to be, is inapplicable, as an explanatory principle, to the structures and functions of the organism, including neural and other physiological processes involved in sensation.

ing, namely, whether a sensation or sensory quality can be wholly accounted for in terms of other sensations or sensory qualities which (on some theories) constitute the physical conditions of the given sensation, or in terms of imperceptible physical entities indicated by such qualities and held (by other theories) to constitute these conditions. We have concluded that such is not the case. Positivism in its orthodox form recognizes as real, or rather as certain, only sensations (or sensory qualities), with their groupings and sequences; and regards as unreal, or as, at best, only convenient descriptive concepts, alleged imperceptible entities, including psychical entities, and atoms, molecules, electrons or other hypothetical elements of matter, which others would hold to be involved, as factors, in sensation. It also denies any validity to the causal principle, except in the same sense of a convenient device whereby briefly to summarize the sense-impressions which are the sole indubitable reality. The implications of the previous discussion as to the validity of this doctrine may be briefly indicated.

Positivism offers no account of the fact that a given sense-impression has as its (relatively) invariable concomitants (or antecedents) a number of other sense-impressions. This refusal to account for such invariable connections is, of course, a cardinal trait of the doctrine and one instance of its general skepticism as to substances, causes or imperceptible entities of any sort. This feature of the doctrine we cannot examine at this point. It is dealt with explicitly, though briefly, elsewhere in this volume; and our discussion throughout is, by implication, critical of it. We shall have to assume here that there *are* necessary connections between a sensation and its concomitant or antecedent physical conditions. This assumption is necessary to our next and substantive criticism of the orthodox positivistic position with respect to sensations.

The physical conditions of a given sensation or sensory quality are, for positivism, only other sensations or sensory qualities, since it does not admit the existence of imperceptible entities back of such sensations. Now, the explanation of a given sensory quality in terms of a number of other sensory qualities sets up an infinite regress of the sort implied by the second type of neo-realistic theory which we examined. This should be obvious and we need not therefore repeat in relation to positivism the argument which revealed the same defect in that type of neo-realism.

Another argument, not available in our criticism of the other theory, may be presented. The sensory qualities which, for positivism, must constitute the physical conditions of a given sensory quality,

are, as a rule, severally very unlike the latter; and not of a sort that by any process of fusion could produce that quality. For example, the sensory qualities of external objects, retina, optic nerve, brain and so on would not, by such a process, produce a red, green or other color correlated with sensory qualities of the kind indicated.

We could also deduce a criticism of positivism from the discussion of the previous chapter. We there showed that factors are involved in action with reference to distant (and, often-times, *imperceptible*) objects that are neither in the nature of sensations nor of imperceptible entities that may be indicated by sensations. This conclusion, if valid, is clearly incompatible with positivism of the present type.

The foregoing criticism of positivism in its orthodox form may be taken as a defense against that doctrine, of our position that mental (or sub-mental) entities are prime factors in sensations or sensory qualities. A systematic defense, against positivism, of our general theory of experience and its underlying assumptions will be presented in a later chapter.

A more recent version of positivism forms an integral part of the doctrine known as radical empiricism, and our discussion of the latter doctrine carries with it such criticism of positivism in that form as we have to offer.

Our discussion of positivism in any of its forms assumes the legitimacy of *explanation* in our investigations of nature or, in other words, the legitimacy of the desire to know *how* relatively uniform sequences in nature are brought about. It further assumes that we have a right to ask the question whether the how of phenomenal sequences lies solely in the observed phenomena themselves; and finally, that if the how in any given case cannot be accounted for without the assumption of imperceptible entities, there is no valid reason for refusing to make such assumptions. As stated, such justification as we have to offer for these assumptions is presented elsewhere in our discussion. If these assumptions are valid our criticisms of positivism naturally follow.

Idealistic Conceptions of Sensory Experience

We have next to examine theories which conceive sensations or sensory qualities as altogether mental in character, and which deny the presence of elements therein which are not mental. At least two modern types of idealism occupy this general position, though their accounts of the matter are otherwise very different from one another.

Since we are taking, in our analysis, the standpoint of the *individual's* sensory experience, we may examine first that type of idealism which conceives sensory qualities (as well as other realities) to be contents of the individual mind. This doctrine, which is subjective idealism, or subjectivism, has been formulated in various ways, and a systematic criticism of it would necessarily take these into account. We shall not, however, undertake such a criticism, but limit our discussion to features of the general doctrine that bear most vitally on our problem.

All forms of subjectivism hold that so-called external objects are only groups of sensations, sense-impressions or sensory qualities and that, as aforesaid, these are purely mental in character. It is not claimed, at least in the more influential versions of this doctrine, that sensations are *creations* of the mind but rather that they are *contents* of the mind, which are or may be induced in our minds by outside causes. Berkeley, for example, held that sensations are produced in us by God or an Infinite Spirit acting on our finite spirits from the outside, while Pearson insists that, so far as the so-called external world is concerned, we can know only the sense-impressions we refer to it, though conceding the possibility of there being an unknowable *plus* beyond those impressions.[1]

Our criticism of this doctrine will take the form of showing that the diversities, sequences and uniform connections in our sensory experience can be accounted for only on the assumption that there is a system of entities external to and independent of the mind, which interact with the latter (on occasion) to produce our sensations and other phases of our experience. If we succeed in doing this, we shall at the same time show that agnosticism as to the possible external causes of sensations is justified only to a limited extent; and that Berkeley's Infinite Spirit or other putative external cause of sensations corresponds in detail to the ordered system of independent entities, which, as we think, can be deduced from our sensory experience. It will be our contention, further, that since these entities are made known to us through our sensations and the analysis thereof (both of which are partially determined by mental or sub-mental factors), we can not know those entities directly, as they exist in themselves. This external, independent world as it exists apart from our minds is the sub-

[1] *The Grammar of Science,* second ed., pp. 67–68. Objection may be raised as to our citation of Pearson as a representative of this type of idealism. But an attentive examination of his writings will furnish ample justification for such a classification. See, for example, p. 75 of the work cited, where the field of science is spoken of as "essentially the contents of the mind," meaning the individual mind.

physical order, as we have conceived it. We will say at once that it corresponds in detail to, though it is not identical with, the world investigated (in part) by the physical sciences.[1]

The grounds for these positions will be very briefly stated. The method of dealing with the problem will necessarily be an *analysis in situ,* because none of the factors in sensory experience are ever presented to us apart from such experience. Only an analysis of this type, therefore, can identify the various sorts of factors in sensory experience and, so far as that may be possible, specify their distinctive characters. It must be borne in mind that we are assuming as already demonstrated, the contribution of distinct mental (or sub-mental) entities to sensations or sensory qualities, and are here only undertaking to show that entities distinct from the mind also contribute to the nature of sensations; and to indicate, in a general way, the nature and order of these entities.

That entities of a sort which exists apart from sensations do induce our sensations is attested by the experience of daily life and by scientific experiment on the conditions of sensory experience. The more significant types of this evidence and the conclusions justified thereby may now be exhibited.

(1) Various sense organs can be temporarily insulated from the sorts of influences which at other times affect these organs, and induce by means of the latter, processes that culminate in sensations. When such insulation is removed, the flow of sensory experience is resumed. The fact that this insulation in no sense destroys or mutilates the organism or any of its faculties shows that sensations are induced by entities which are not a part of the organism or its faculties, though the sensations thus induced do initiate more or less durable modifications of the organism. This analysis would not apply, without qualification, to organic and motor sensations, since the influences inducing these sensations come from within the organism. These influences, however, include elements which are but temporarily parts of the organism. Since these extra-organic influences are as essential to the occurrence of sensations (at least external sensations) as are organic factors themselves (including mental factors), we are obliged to assume, in the absence of decisive proof to the contrary, that they contribute to the nature of sensory qualities experienced by the organism.

(2) The type of experiment just indicated, when repeated the requi-

[1] The qualification "in part" refers, not to the unexplored regions of the physical world recognized to fall within the domain of the physical sciences, but to features of that world which are signified, or possibly signified, by the organic and motor sensations, though hitherto unrecognized as such by the physical sciences.

site number of times, and under the requisite diversities of circumstance, shows, or would show, that the sensations induced by extra-organic influences are of many different kinds, of as many kinds, in fact, as human beings have experienced. This fact in itself might perhaps not prove that the qualitative diversities in our sensory experience are due, in part, to these extra-organic influences. We earlier adduced evidence showing that sensory qualities derive part of their quality from mental factors of the organism experiencing these qualities. Might not all their quality be due to the same factors? The fact that, with the consent of the given percipient organism (the subject), an experimenter may (within limits) cause to be induced in the former sensations of any quality whatsoever, by applying to his sense-receptors the appropriate sorts of extra-organic influences, tends to show that all sensory qualities derive their several distinctive qualities, in part, from the correlative external influences. The fact that these external influences are largely subject to the control of individuals other than the given percipient organism, and that many of them may be stored, transported or otherwise controlled *without inducing sensations in any one* proves at least that these influences as such are entirely independent of sensory experience. May we not legitimately infer from these facts that all sensations derive part of their qualities from influences entirely independent of the organism, save on the occasion of sensory experience?

It is conceivable, however, that the qualities of sensations might be due entirely to characteristics of the sensory nerves, or to the mental (sub-mental) factors of the organism, or to factors of both sorts combined, with the external stimuli functioning only to arouse organic processes of the sort indicated in such a way as to produce the several sensations correlated with external stimuli. The discredit suffered by the doctrine that the sensory nerves are characterized by specific energies, and the absence of visual or auditory sensations in the case of persons born blind or deaf do not seem to exclude these possibilities; since peculiarities of the sensory nerves might be discovered which would reinstate one of these possibilities; while the mind at birth might be endowed with capacities for generating all the various sensations, but external stimuli might be required to set the several generative processes in motion, which would suggest the other possibility. But since external influences are as essential to the generation of sensations as are organic factors, we are obliged to assume, until the contrary is proved, that these influences contribute to the qualities of the sensations severally induced thereby.

(3) We experience sensations of particular sorts at various places,

and observe changes in the sorts of sensations experienced in the same place at different times; and we are enabled, through the coördination of such experiences (on the part of many observers), to refer the various extra-organic entities capable of inducing sensations to definite positions in the given space, and to correlate changes in those entities with the time order.

The fact that many sensations experienced by us are found to be arranged in more or less uniform sequences, taken in connection with the fact that sensations owe a part of their qualities to extra-organic entities, shows that the extra-organic entities which induce the sensations of those sequences are themselves ordered in a similar manner.

Such sequences are taken by some, though not by others, to signify *necessary* connections between the component elements of those sequences. All schools of philosophical thought designate such uniform connections by the term causal relationships, though ascribing very diverse meanings to this term. These causal relationships are themselves correlated in various ways with spatial and temporal relationships, though by no means (as we should hold) identical with the latter.

All this evidence taken together seems to justify the conclusion (1) that all our sensations are induced by entities existing prior to these several sensations and (before the latter are induced) independently of our minds; (2) that the qualities of all our sensations are due in part to the nature of the several entities that induce them; (3) that these entities are localized, and therefore enter into a spatial order; that they undergo changes of various sorts and therefore enter into a temporal order; and that they give rise to sensations arranged in (relatively, uniform sequences, and therefore enter into a causal order. These entities as thus coördinated constitute what, according to our doctrine, may be termed the sub-physical order. It is not implied that all the entities of this order induce or are capable of inducing sensations. Indeed it is certain that the major portion of those entities are practically inaccessible to sensitive organisms, and hence do not induce sensations of any sort; and it seems very probable that many types of sub-physical entities are not *capable* of inducing sensations at all.

We do not here raise the question whether or in what sense time and space themselves subsist independently of our sensory experience, or of the sub-mental and sub-physical factors which enter into that experience. This question will be briefly considered at a later point in the discussion.

In order not to complicate the preceding analysis, we took for granted the several contributions to the nature of sensory qualities by the extra-organic entities whence the stimulus originates; by the

medium whereby influences from those entities are transmitted to the sense-organs (those influences in many cases being succeeded, in the medium, by influences of a different sort); and by the sense organs themselves, the sensory nerves and the central nervous system. All these, according to our conception, would embrace component elements from the sub-physical order, though entering our experience in the form of physical complexes, in which sub-mental entities are also component elements. And the physiological processes appear to embrace component elements, as we shall see later, that are neither sub-physical nor sub-mental, though perhaps coming with the latter under a broader category, of which these two classes are similar though different varieties. We refer here to factors involved in the maintenance of organic structure and the organization of physiological activities, which seem irreducible to terms of physicochemical processes, or of mental (sub-mental) factors such as enter into our conscious experience. The possibility that still other types of entities are involved in sensory and other phases of our experience will be considered later.

The evidence that extra-organic entity, medium and physiological processes severally contribute to the nature of sensory qualities was presented in connection with our examination of neo-realistic theories of sensation. In that discussion we did not indicate the more positive evidence for the conclusion that the physiological processes which condition sensation make a contribution to the nature of sensory qualities, arguing only that their indispensability to sensations justified this conclusion, unless conclusive evidence to the contrary should be produced. Specific evidence that physiological processes do contribute to the nature of sensory qualities is found in the fact that the impulse of the sensory nerve, though its nature has always been something of a mystery, is certainly very different from the external stimulus which induces the impulse. For the impulse at least includes chemical and electrical phenomena such as cannot be attributed to the stimulus. Since this fact is well recognized, we need not dwell on it further. Whether the sensory nerves and central nervous system make contributions to sensory qualities distinct from that of the sense organ cannot be determined on the basis of the available evidence. Their contribution, for aught we know to the contrary, may be the purely instrumental one of transmitting and coördinating the various sensory impulses initiated, we must believe, through some sort of interaction between the stimulus and the sense-receptor. But it is by no means certain that this is the case.

Unless theories of sensory qualities still to be examined should necessitate serious qualifications of the foregoing analysis, it would

seem to justify our doctrine that sensory qualities, together with the physical objects and events constituted by their groupings and sequences, are the resultants of interactions between sub-mental and sub-physical factors, the latter including the entities originating the stimulus, the intervening medium and physiological processes of the organism. (As before stated, other types of entities are also involved in sensory experience.)

If these various factors all contribute to the nature of sensory qualities, and hence of physical objects and events, none of those factors are ever given pure in our sensory experience. Hence the propriety of *not* designating the extra-mental factors in the physical object as themselves physical entities, but of giving them a distinguishing term, as, in our terminology, sub-physical entities. (It must be remembered that extra-mental factors in physiological processes embrace, as we are to show, organizatory factors which are not sub-physical entities.) Hence, also, the propriety of not designating as mental the organic factors in sensory experience other than those involved in physiological processes, since in this case the mental, if it can be defined at all, embraces the entire sensory experience and therefore contributions of sub-physical factors to that experience. Sub-mental entities is the term we have adopted whereby to designate this group of organic factors.

The two types of idealism termed, respectively, objective idealism and absolute idealism or, more briefly, idealism and absolutism, would not seem to carry any implications seriously affecting the analysis set forth in the preceding pages, other than to offer *additional* interpretations of the results yielded thereby. They have not, so far as we know, developed an analysis at all similar to ours, but there would seem to be nothing in them radically incompatible with the results of our analysis. These remarks may now be elaborated and qualified so far as seems necessary to the proper placing of our doctrine with respect to these types of idealism. We shall discuss only features of the two doctrines which bear most vitally on the problem of sensory experience.

According to objective idealism, physical objects do not depend on the momentary perceptions of finite minds, but are the enduring content of a Great Mind which is in continuous cognitive relations with physical and all other phases of reality. The doctrine does not imply that this content is an intrinsic part of the Great Mind itself, but is something it works on and, through its own creative power, orders or gives laws to.[1] This doctrine is, in fact, accepted on the ground that the Great Mind or Self "accounts for the well-ordered world

[1] Sheldon, W. H., *Strife of Systems and Productive Duality*, p. 108.

of objects," as well as on the ground that "our inner consciousness bears witness to the presence of that Being." [1] Many other arguments are of course adduced in support of this general doctrine, especially arguments deduced from the underlying reality theory of relations, upon which this type of idealism is based; [2] but these arguments seem to have no special bearing on our present problem, and need not be considered in our further discussion.

Another doctrine advocated by many idealistic philosophers is that the *contents* of the Great Mind which constitute inorganic nature are mental or vital in their own right, though differing, as to these characteristics, from living organisms. Royce, for example, by emphasizing certain very general analogies between so-called conscious and material nature, respectively, seeks to minimize the difficulties in the way of conceiving the latter as itself conscious; and proceeds to build up a theory to the effect that "in case of Nature in general, as in case of the particular portions of Nature known as our fellow-men, we are dealing with phenomenal signs of a vast conscious process, whose relation to Time varies vastly, but whose general characters are throughout the same." [3] Similar conceptions have been advanced by Ward,[4] Taylor [5] and other idealistic philosophers, though such conceptions do not seem to be an essential part of the general doctrine under consideration, in the sense of being implied by the fundamental positions of the latter.

Let us consider first the doctrine sketched above, that physical nature is content of a Great Mind, as well as (partially and intermittently) of finite minds. So far as we know, no philosopher of this school has explained just how or in what sense physical objects are contents of a Great Mind or of finite minds.[6] Are such objects *per se* contents of any such mind, or do only their phenomenal signs bear this relation to the latter? If the content of the given mind is one of phenomena only, are these produced in the mind by the physical ob-

[1] *Ibid.*, p. 116. It should be stated perhaps that Sheldon, while a sympathetic exponent of this type of idealism, is also a vigorous critic of it.

[2] Cf. Spaulding, E. G., *The New Rationalism,* Chap. XXXV.

[3] *The World and the Individual,* Vol. II, pp. 209–233; quotation from p. 226.

[4] *Naturalism and Agnosticism,* Part V.

[5] *Elements of Metaphysics,* pp. 208–215. Taylor at least is properly classifiable as a member of the absolutistic school, and Royce has also been referred, by some, to the same group. Hard-and-fast classifications are scarcely possible in this field and, at any rate, are not essential to our purposes. We are concerned only to examine idealistic doctrines which have a significant bearing on the problem of sensory experience, whatever the proper grouping of those doctrines may be.

[6] We speak on this point, subject to correction, as our acquaintance with the literature of this school is far from complete. We proceed, however, to examine the various interpretations of the relation between mind and physical object that would be severally consistent with the basic doctrines of this type of idealism.

ject alone, or by the interaction of the object with the mind of which the phenomena are contents? Idealistic writings seem to leave open these several alternatives, and we may therefore indicate the bearing of our previous analysis on each of them in turn.

If the general doctrine means that physical entities as they exist in themselves are contents of finite minds or of a Great Mind, then it would be subject to the criticism which we preferred against the neo-realistic theory of sensory qualities represented by Holt. The same criticism would apply if the doctrine be interpreted to mean that phenomena emanating complete from the physical object become contents of the percipient mind. If, on the other hand, the doctrine means that the physical object as perceived becomes an exclusively *mental* content of the percipient mind or minds, it is open to the criticism applied by us to the subjectivistic doctrine of the physical world. If, finally, the doctrine means that extra-mental entity and percipient mind both contribute to the qualities of the physical object, it is the equivalent, insofar, of the doctrine we have advanced.

Some qualifications of these conclusions will be in order. First, the postulation of a Great Mind which has the physical world as content does not appear to exempt the doctrine from the criticisms advanced by us against the subjectivistic or the first type of neo-realistic account of sensory qualities, respectively, according as the relation between the physical world and that mind be construed in one or the other of these ways. For an intelligible conception of that mind must represent it as of somewhat the same nature as finite human minds, particularly so far as its experience of sensory qualities and physical objects is concerned. If this be denied, then the doctrine wants further development by showing just how the Great Mind differs from finite minds as regards its relation to the physical world.

If the general doctrine be interpreted as similar to our own doctrine in the respects indicated, the question arises whether entities termed by us sub-physical are properly conceived as conscious beings or processes. That question may, so far as it bears on our doctrine, be disposed of very summarily. Whether we shall ever know enough of the entities that constitute inorganic nature to justify their inclusion in one category with living or, more particularly, conscious organisms, cannot now be decided. The two sorts of beings appear at present to be so radically different, that to designate them by the same term is hardly more than a modification in the usage of mere words. If we call them both conscious, or material, or by any other common name, we shall have to employ other terms whereby to denote the, at present, irreducible differences between the two. No qualitative monism has ever ex-

plained away these differences; all such monisms are in fact obliged to recognize and allow for them in one way or another. Of course men will some day know more about the "real" natures of the two types of entities, and this knowledge may reveal closer affinities between the two than any we can now demonstrate—so close, possibly, as to justify some sort of qualitative monism; but that time is not yet, and we would best not pretend to a synthesis of this sort that empirical evidence does not at present justify. Royce, himself, despite the analogies between the two types of entities which he stresses, recognizes differences so great that his analogies become decidedly "thin" by comparison. Any such doctrine as his must be unconvincing when squarely confronted by these differences.

The doctrines of absolute idealism raise fewer questions pertinent to our analysis than do the doctrines of objective idealism, and we can therefore be briefer in dealing with them. Absolute idealism does not single out any aspect of reality as existentially supreme over all other aspects, but, to take a significant example, concedes some degree of reality to physical objects apart from a mind or minds with which they may be related. A basic contention of the doctrine, however, is that all such aspects of reality are but appearances of an all-inclusive Absolute,[1] or else, while real as appearances, are given the highest degree of unity and meaning only in the Absolute.[2]

The conception of the Absolute is based upon the principle of the internality of relations, according to which any entity is essentially constituted (in part) by its relations with other entities; and upon the further principle that each particular in the universe implies and is implied by all other particulars; this latter principle indicating the scope of the relations which are partially constitutive of each individual entity. These principles, when developed, give an organic whole which, while embracing its component elements and their relations, synthetizes or transmutes these into a higher unity. This doctrine is held to be idealistic on the ground that only a Mind can fulfill the specifications indicated. But the Absolute Mind is not mind in an empirical sense, though logically similar to such mind.

It is not claimed that a particular entity is constituted alone by its relations to other entities, but by these relations and something unique in addition, something over and above its relations. And while it is admitted that the universal implicativeness of things cannot be demon-

[1] Bradley, F. H., *Appearance and Reality,* and other works.

[2] Royce, J., *The World and the Individual, passim,* which may be taken as also an exposition of absolute idealism. Bosanquet in *The Principle of Individuality and Value* and other writings takes a position intermediate between those occupied by Bradley and Royce.

strated in detail, it is claimed that both logical motives and the numerous empirical relationships between things justify us in accepting this conception.

This summary statement of the doctrine under consideration will perhaps suffice for our purposes. It does not as represented here seem to carry any very critical implications for our doctrine except that, on Bradley's version of the doctrine at least, our sub-physical and sub-mental entities, though not in the nature of appearances, would be held not to possess a very high degree of reality; for advocates of the Bradleian doctrine would doubtless contend that these entities, granting their existence, implicate one another (as well as other things), and therefore derive part of such reality as they may possess from these implicative relations.

Some remarks on these absolutistic implications as to our doctrine may be offered. If we proceed on the basis of the empirical evidence and of the inferential conclusions adequately supported by that evidence, we seem hardly justified in asserting that sub-physical and sub-mental entities are related except when interacting in sensory or other phases of our experience. Of course a considerable portion of sub-physical entities would be capable of entering into such interactions, were organisms brought within perceptual range of them; but it seems likely that a great many sub-physical entities are not capable, under any circumstances, of entering as factors into our sensory experience. Again, sub-mental entities are always combined in organisms, so far as we know, with sub-physical entities, but they are not of course combined with all such entities or capable, so far as is known, of being combined with more than a few sorts of them.

The absolutistic rejoinder to these arguments would doubtless be that sub-physical and sub-mental entities nevertheless implicate one another throughout, since, for example, each of the two types of entities is defined in relation to the other type. Our reply would be that while this is true, such definition is made necessary by the conditions of our experience—neither type of entity ever being given pure in that experience—and that this does not imply that sub-mental entities are in any sense constituted by the sub-physical or *vice versa*. Indeed a basic contention of our doctrine is that the two types of entities are ontologically heterogeneous, and that by far the greater portion of sub-physical entities at least *exist* in entire independence of sub-mental entities, even though *defined* in relation to the latter. And this result would not appear to be vitiated by the fact that the mind (with its sub-mental entities) forms a conception of sub-physical entities generally, embracing all the individual members of this class. For such a

conception, with all other conceptions of physical entities added, could scarcely refer to each of these individual entities; and, even if this could be so, that fact would hardly contribute anything to the nature of those entities. The same statement would apply, we believe, to relations of similarity and difference as well as other types of logical relations among these entities.

Our object in offering these observations is not a polemical one, for we are not here undertaking to appraise absolutistic doctrines, but only to guard our conception against what might be deemed prejudicial implications of those doctrines. We could indeed grant the basic positions of the absolutist, and still hold our own doctrine to be valid in the context defined for it. It should suffice to point out that this is so and to show that our analysis lends little if any support to those features of the absolutistic theory which we have considered.

We would add, in conclusion, that advocates of this theory have, in our judgment, made a most important contribution to philosophy in their destructive criticism of what we would, with them, deem one-sided conceptions of sensory qualities, as well as of matter, mind and mind-body relationships.[1] To Bradley's work on this side, in particular, we acknowledge our own indebtedness. It is mainly in regard to the positive conceptions based on this destructive criticism that we differ from members of this school. Whereas they proceed to develop an ultimately agnostic position as to the nature of reality, affirming, as they do, that only a partially knowable whole is real in the fullest sense; we, on the contrary, while acknowledging the impossibility of directly knowing various types of factors in experience, as these exist independently of experience, would not go beyond such specific syntheses of heterogeneous factors as are demonstrated by the empirical evidence and its analysis.

The Radical Empiricist's Conception of Sensation

We may now consider, from the standpoint of the results thus far achieved, the doctrines of radical empiricism which bear specially on the problem of sensory experience, confining our remarks in this connection to James' elaboration of these doctrines.

James insists, as we have already noted, that thoughts and things, or states of consciousness and physical objects, are the same materials of experience taken in different contexts; that the distinctions between these pairs of attributes are not ontological, but functional or relational,

[1] Notably Bradley in *Appearance and Reality* and, more recently, Hoernlé in *Studies in Contemporary Metaphysics.*

in character. This contention we discussed in the previous chapter, but it will be necessary to consider it further, in relation to the present problem. Another of James' basic doctrines is that the relations between the various bits of experience fall, as such, within experience itself, in the same way as do the bits of experience related; and that these relations are not therefore the work of the mind in any sense that the bits of experience themselves are not. Radical empiricism is differentiated from ordinary empiricism (orthodox positivism) and from rationalism by this insistence on the objectivity of relations. The doctrine holds, in short, that nothing is to be admitted as fact which cannot be experienced at some definite time by an experient.

The deeper features of reality, according to James, are found only in perceptual experiences, though these are rounded out by conceptual experiences. Conceptual knowledge is such by virtue of things that fall outside the knowing experience itself, but these things are intermediary experiences leading up to a terminus (a perceptual experience) that completes such knowledge. Most of our knowledge is never completed or "nailed down" in this sense; it does not get beyond a *virtual* stage. Otherwise stated, unchallenged thinking is, in the great majority of instances, our practical substitute for knowing in the completed sense. This applies not only to our ideas of imperceptibles like "ions" or contents of our neighbors' minds, but to ideas which could be verified, yet which we accept as true without verification, unless opposed by some contradictory truth. With all this assumed experienceability of things and their relations, we should be willing to admit "noumenal beings or events . . . if only their pragmatic value can be shown." [1]

The critical implications of our analysis for these doctrines will now be indicated, particularly as they bear on the problem of sensory experience. Our analysis showed, as we believe, that extra-mental source of the stimulus, events in the intervening medium (for the external sensations at least), processes in sense organs, sensory nerve and central nervous system, and mental or sub-mental factors all contribute to the nature of sensory qualities. If that be true, none of these factors are ever given pure in sensory (or other phases of our) experience, and the doctrine is untenable, that every thing admissible as fact is capable of being experienced somewhere by an experient. Possibly James would have conceded that those alleged factors in sensory qualities are *virtually* given in experience and, if so, we should have no further quarrel with his doctrine on this score; but if he at the same time held

[1] *Essays in Radical Empiricism,* p. 242. Most of the foregoing sketch is based on the Preface and first chapters of this book.

consistently to his basic doctrine that all real things and relations are capable of being directly experienced, he would have combined together two incompatible doctrines.[1]

James' doctrine would appear to imply a neo-realistic theory of sensory qualities, since he repudiates any ontological distinction between mind and physical things, besides insisting on the superior cognitive value of perceptual as compared with conceptual experiences. Perceptual experiences would reveal only the physical components of sensory qualities, those investigated by physics and physiology; and from these, so far as we can see, the theory would have to derive the sensory qualities. The contention that physical objects and states of consciousness, or thought and things, are but the same bits of experience taken in different relational contexts affords an additional and conclusive justification for this construction of the theory. If this be so, the same criticisms apply to it as to the interpretation of sensory qualities yielded by the neo-realistic theory in one of the two forms examined, depending on the form toward which the present doctrine might incline in its account of sensory experience. The doctrine does not appear to lend itself to a subjectivistic construction, as regards its implications for the problem of sensory experience, since it rejects the concept of mind or consciousness as a type of entity distinct from material things.

We need only add to this summary statement, the fact that parts of experience which do *not* enter into purely physical (or inanimate) manifolds are essential factors in the series of experiences which condition the behavior of animal organisms, as the discussion in the previous chapter undertook to show. Only by taking these (the cognitive and purposive) parts of experience into consideration, can we account for the differences in the relations within groups of perceptual experiences, wherein the activity of animal organisms is and is not respectively an essential factor. Otherwise stated, the order of perceptual experiences vitally depends on the presence or absence therein of overt activity on the part of an animal organism, and this activity is determined in part by factors which are not in the nature of sensory experience, or of the physical objects constituted or indicated by such experience. This added comment refers only to the order of, or relationships between, sensory experiences, not to the analysis of specific sensations or

[1] James does not appear to have committed himself definitely on the question whether or no alleged imperceptible entities are ever to be credited with reality, though he concedes that they should be admitted provided their pragmatic value can be shown. He appears to assume, however, that such value cannot be demonstrated.

sensory qualities. As to the latter, the present doctrine would properly be construed, as stated, in a neo-realistic sense, the criticism of which has already been presented.

The foregoing critique of James' doctrine does not beg any question pertaining to causation, since he assumes the reality of causal relations, which, as in the case of other types of relations, he interprets in a radically empirical fashion, insisting that they must be directly experienceable *as* causal relations.[1] While we should not ourselves accept this view of causal relationships, it is not in itself repugnant to the type of criticism which we have advanced against the theory of sensory qualities implied by James' doctrine, though the results of our criticism call in question the validity of his view of causation.

Critical Realists' Conceptions of Sensory Experience

We shall close our analysis of sensory qualities and the factors therein by pointing out its implications for certain conceptions of the subject which come within the body of doctrine known as critical realism. Our own position, epistemologically considered, conforms to the general standpoint of this doctrine, since, in common with the critical realists, we hold that physical existents (our sub-physical entities) are not, as such, given in perceptual experience. We should add the equally important doctrine—one apparently not recognized by this school—that purely mental (our sub-mental) entities or processes are not given, as such, in our experience.

Critical realists differ, of course, in the details of their several epistemological conceptions, and quite widely in the ontological doctrines combined with these conceptions. These differences we cannot discuss systematically, but will confine our observations in this connection to certain doctrines held by members of this school which appear open to criticism.

Some critical realists hold that the data or contents of sense-perception are altogether mental in character.[2] This position would obviously be disallowed by our analysis of sensory experience, showing, as it does, that, in addition to mental (or sub-mental) factors therein, the sub-physical entities constituting the source of the stimulus, the events in the intervening medium, the sense organ, sensory nerve

[1] *Op. cit.,* pp. 181 ff.

[2] Arthur O. Lovejoy, James Bissett Pratt and Roy Wood Sellars in their respective contributions to *Essays in Critical Realism;* see in the same volume, pp. 4, 20–21, footnotes. Sellars' position on this and other epistemological problems is further elaborated in his *Critical Realism, passim,* and *Evolutionary Naturalism,* Chaps. II and III.

and central nervous system are also involved in sensory experience; and that all these have a logical claim, equally with sub-mental factors, to be regarded as contributors to the nature of sensory qualities. Further elaboration of this criticism may here be dispensed with, as we may refer to the previous discussion for such justification of it as we are now prepared to offer.

Other critical realists make their epistemology turn on the concept of essence,[1] a logical entity which is not intrinsically physical or psychical in character, but which is a quality, idea or other universal that may be given to sense or to thought. Certain sorts of essences by intuition or some other psychical process get themselves given in sense-perception, and are or may be affirmed to be essences of external objects perceived by sense.

The machinery whereby essences mediate between the organism and the external world is made very intricate, especially in Strong's theory, and we cannot here undertake to give any adequate report of it. Just how the external object and the organism function in calling up the essence and, above all, how the essence itself, which as such does not exist, can at the same time be the character of a physical object which does exist are not made very clear. It seems, in fact, to be an act of faith, an instinctive affirmation, an implicit assumption (such are the terms used) whereby the essence is referred to the external object as its character. And the validity of such reference is conceded to be rather precarious, even in cases of what is regarded as genuine perceptual knowledge of the physical world.

The point upon which we would comment, however, is the claim that the essences, *e. g.*, secondary qualities, given in sense-perception may be and often are identical with the essences of the external object. This claim would obviously be disallowed by our analysis, so far as sensory qualities are concerned, since it purports to show that these qualities are syntheses of factors contributed by many other conditions of sensory experience besides the physical (or sub-physical) entities whence the sensory stimulus originates. For the detailed justification of this criticism we must refer to our analysis of neo-realistic and subjectivistic theories of sensory qualities previously set forth.

The only other doctrine held by members of this school upon which we would comment here is that mental factors are to be identified with physical conditions, particularly the brain and its processes. Our com-

[1] Durant Drake, Arthur K. Rogers, George Santayana and C. A. Strong, in their several contributations to *Essays in Critical Realism;* also Strong, *The Origin of Consciousness;* and Santayana, *Scepticism and Animal Faith.*

ments on this doctrine will have reference to Sellars' exposition thereof.[1] Since Sellars' conception of the mental was criticised in the previous chapter,[2] we will here consider it only in relation to the results yielded by our analysis of sensory qualities. The only serious criticism justified by these results is that Sellars makes the mind ontologically dependent on the correlated neural processes, and fails to recognize that it embraces entities distinct from those synthetized in these processes.

Notwithstanding these criticisms, Sellars' epistemological analysis of sense-perception is, in our judgment, superior to any other one in the current literature. For he recognizes all the main conditions of sensory experience, mental factors included, without minimizing the contribution made by any of them. We would even defend him against his own doctrine, that the mental is to be identified, ontologically, with its correlative physical conditions, since, in practice, he deals with the mental on its own terms and recognizes fully the part it plays in the experience and behavior of the organism. His purported identification of the mental with the physical is scarcely more than a verbal one. And while, as noted above, we disagree with him in making the content of perception altogether mental in character, his own analysis hardly supports this view of the matter. Sellars has, in short, presented an analysis justifying in detail the type of ontological doctrine herein expounded, though he would doubtless not agree to this construction of his results.

The members of this school generally have appreciated better the difficulties in the problem of the relation between the mind and the external world, than have any other group of modern philosophers, with some possible exceptions among the absolute idealists. While certain of the solutions proposed for this problem do not impress us as being promising ones, to conceive the problem rightly is to have made genuine progress toward its solution. We believe that further progress in this direction depends largely on working out more thoroughly the metaphysical implications of the epistemological position taken by the critical realists, and in turn employing the metaphysical conceptions thus elaborated in the further development of the epistemological position. Professors Pratt and Lovejoy alone of this school appear to be headed in the right direction, as regards a well grounded body of ontological doctrine, holding, as they frankly do, to a dualism of matter

[1] *Evolutionary Naturalism,* Chaps. XIV and XV, and *Critical Realism,* Chap. IX, and other writings.

[2] *Supra,* pp. 204, 207–208, 221–224.

and spirit; though neither of them have recognized that both these terms, in their more common meanings, connote complexes of radically heterogeneous elements which must be analyzed out before secure foundations for metaphysics and epistemology can be laid.

Further Analysis of Sub-Physical and Sub-Mental Factors

Herewith must end the elaboration and defense of our conception of sub-physical and sub-mental entities, by way of an analysis of sensory qualities and conceptions thereof in conflict with our own. We must leave to the future the task of investigating in detail the types of interactions whereby the sub-mental and the sub-physical determine sensory and other phases of our experience. In the remainder of this chapter, we shall undertake only to specify some of the general relationships between the two types of entities, and analyze briefly the rôles played in our experience by entities not classifiable under either of these categories.

This will be done in the main without explicit reference to the implications of current philosophical doctrines for our own doctrine as thus elaborated. The further exposition of our doctrine can be presented briefly and independently of rival doctrines, because conceptions of the mental, the physical and their relationships must be grounded primarily on an analysis of sensory experience, wherein the vast and intricate commerce between the mind and the external world has its start. Some elaboration of the doctrine in relation to other phases of experience, however, is essential to the development of its broader implications respecting the primary factors of experience in general. We shall first attempt a more explicit characterization of sub-physical and sub-mental entities, respectively, and then go on to consider some of the more significant types of syntheses assumed by these entities.

To begin with, the sub-physical and the sub-mental may be characterized negatively, each with respect to the other. We should say that to each of the two must be attributed those features of our experience which cannot be accounted for in terms of the other (always allowing, be it remembered, for factors in our experience not coming within either of these categories). We may assert, on the basis of the previous analysis, that, without the sub-mental, the physical would be impossible, and that the sub-physical could not be known or inferred to exist; and likewise that without the sub-physical, the mental as

we conceive it would be impossible, and that *perhaps* the sub-mental could not be known or inferred to exist. These negative definitions are justified, so far as they go, by the preceding analysis.

From these negative characterizations we can proceed to conceptions with a more positive content. The specific types of experience possible to us can be arranged in a series approaching at either end the sub-physical and sub-mental orders, respectively, though stopping short of the limits set by these orders; and comprehending within the series indefinitely varied syntheses of sub-physical and sub-mental entities, each representing a qualitatively and in some sense quantitatively distinct combination of components from the two categories. As aforesaid, the mental as ordinarily conceived contains a larger element of the sub-mental than of the sub-physical, and the physical a larger element of the sub-physical than of the sub-mental. Instances approaching the limits at either end would be, for the sub-physical, the entities inferred to exist at positions in space beyond the range of our most powerful telescopes, or the ultra-microscopic entities, such as electrons, atoms and electromagnetic vibrations, inferred to exist, from phenomena which are given in our experience; and for the sub-mental, the deduction of the implications of a hypothesis, the analysis of reflective thought, and the like.

Many of the recent movements in the physical sciences have represented a search, although not recognized as such, for the sub-physical order, or at least a notable progress toward the conception of this order. One motive underlying these movements has been an interest in the nature of the elemental components of matter and energy, and the processes whereby these are organized into physical systems. These researches are really concerned with the sub-physical order, though the terms employed in thinking about it have physical connotations. Some of the more philosophical students in this field are indeed beginning to recognize that the images employed in thinking about the physical world have no application in that field.[1] Another motive of recent work in this field is the desire to expose the relativistic nature of previous physical conceptions (particularly in reference to space and time), and work out substitute conceptions which shall be valid for all points of view from which the physical order may be observed. It should be obvious that this movement is likewise tending in the direction of the sub-physical order as we have conceived it.

Recent work in psychology does not, with some exceptions, exhibit a parallel movement. The dominant tendency here is in the direction

[1] *E.g.*, Bertrand Russell in *The ABC of Atoms.*

of interpreting the mental in terms of the physical, and thus of assigning to the sub-mental factors in our experience a status of secondary significance only. The exceptions consist of investigations to elucidate the processes of reflective thought, the genesis of concepts, etc., or, more generally, all the types of research based on the assumption that the mental is largely heterogeneous with the physical and must be approached in a different way. The two tendencies would be represented, roughly speaking, by behaviorists who repudiate (ostensibly at least) the introspective method of investigation, and psychologists who insist on the indispensability of this method, while conceding the value of behavioristic methods.

Perhaps the nearest approach hitherto made toward a conception of the sub-mental order is the attempt to define a category of hereditary mental traits which are indifferent, as such, to any particular physical or sub-physical order which might constitute the "natural environment" of the organism, and indifferent, too, to particular concepts, traditions, ideals, institutions, arts, etc., which might constitute its "cultural environment." This attempt is concerned primarily to identify the general types of mental processes—inference, judgment, genesis of concepts, instinctive tendencies, habit formation, etc.—exhibited in individual development under all sorts of environmental conditions, physical or social.

The validity of such a category is attested by the fact that the human mind always functions, whatever its environment, in ways which may be classified according to general types of mental processes, such as those specified. Individual development is largely determined, of course, by the environing factors and, we are obliged to believe, by the *particular* sub-mental outfit with which the individual is born, so that the particular concepts, interests, etc., actually developed depend on hereditary and environmental factors that are highly variable; but these facts are not repugnant to the concept of general types of mental processes to which the genesis of such concepts, interests, etc., is partially referable. These universal types of mental processes, as distinguished from the specializations thereof in the individual, come within the sub-mental order, as do also the variations thereof which characterize the sub-mental outfits of different individuals. These sub-mental factors are active throughout the life of the individual, though continually reorganized through their interactions with the environment.

In specifying these approaches to the sub-mental and the sub-physical orders, it is not implied that these are ever apprehended in their pure

forms; nor that the sub-mental in particular can exist independently of the sub-physical.[1] Thinking processes approaching most closely to the sub-mental as a limit are always correlated, so far as we know, with a brain and nervous system embodied in sub-physical entities, and are affected in manifold ways by physiological processes in the body generally. Moreover, such thinking processes largely have reference to sub-physical entities appearing to us in the guise of the physical, that is, of perceptible bodies and their movements. And, finally, thinking processes are conditioned by, if not impossible without, speech or other bodily reactions, while they are quite dependent for their communication and hence their development through the coöperation of other minds, on such reactions, as well as on complicated series of sub-physical events in a transmitting medium.

One could of course demonstrate the dependence on sub-physical factors, of other mental processes more or less closely approaching the sub-mental as a limit, such as attention, memory and imagination. These, too, are correlated in manifold ways with the nervous system and the body generally, and largely refer to sub-physical entities partially constitutive of the individual's past or present environment. Sensations, perceptions, feelings and emotions are even more intimately dependent on the sub-physical than are the types of mental processes already specified; representing, in fact, such an intimate combination of sub-physical and sub-mental entities that they cannot properly be designated as either mental or physical in character.

A similar analysis applies to the types of experience which approach most closely to the sub-physical as a limit. These cannot, in our thought, be stripped entirely of their observed or imputed sensory qualities, even though sub-physical entities inferred to exist, as the atom, the electron and the magnetic field, do not as such fall within the limits of sensory experience. These all come to us by way of sense data, and imperceptible entities inferred from these data are never apprehended as they exist apart from sensory experience. The concept of the sub-physical itself is gradually being worked out by complicated methods of analysis, in which the more significant factors are of the sub-mental order. We may say, without further illustrations, that no sub-physical entities are ever apprehended by us as they exist in themselves, though the very concept of these entities means that they are capable of existing apart from sub-mental entities of any sort.

[1] However, consideration of the evolutionary conditions under which sub-mental were combined with sub-physical entities would seem to support the latter proposition. See pp. 395–396.

We might take those phases of our experience standing closest to the sub-physical and sub-mental, respectively, as criteria whereby to identify the specific functions of the two in any of our experiences. Inorganic bodies and their motions may be taken as roughly approximate criteria of the presence and operation of sub-physical entities, if due allowance be made for sub-mental factors in the sensory qualities through which those bodies and their motions are made known to us. Stated differently, we are justified in assuming that the types, concretions, positions and movements of sensory qualities entering into our experience all have their correlates in the sub-physical order and that there is, in fact, some sort of detailed one-to-one correspondence between the two systems. Only on this assumption can the detail, order and variation of sensory qualities experienced by us be accounted for. And we may reasonably assume that the account given by the physical sciences of the physical order holds, in some correspondent fashion, of the sub-physical order also. In any case, this account, when qualified in the ways indicated, gives us our principal criteria of the sub-physical order, and it supplies a sort of negative criteria for the sub-mental order as well.

In a similar fashion we could take those phases of experience which stand closest to the sub-mental, qualify them by discounting for the characters therein due to sub-physical factors, and employ them as positive criteria of sub-mental factors in our experience, as well as negative criteria whereby to identify the correlative sub-physical factors. Examples of experiences standing close to the sub-mental have already been cited.

By utilizing the two sets of criteria, in both their positive and negative applications, we could progress far toward a comprehensive analysis of our experience into its sub-physical and sub-mental determinants. Such an analysis will no doubt require supplementation by other types of analysis, especially for those phases of experience that occupy a middle position between the two limiting orders. But the criteria indicated would not be altogether void of utility in the analysis of those phases of experience. Here, as already indicated, would come sensations, perceptions, feelings and emotions. These phases of experience might be likened to a watershed, on either side of which the streams of experience run in the direction of the sub-physical and sub-mental orders, respectively. The supplementary types of analysis requisite to the resolution of these classes of experience into their sub-physical and sub-mental components are already being worked out, as is illustrated by the discovery of the component elements of stimuli to visual and auditory sensations, the discrimination of the organic and motor sensa-

tions entering into emotional states, the identification of the physiological processes and physicochemical conditions involved in those groups of sensations, and so on.

We shall not undertake here to formulate methods of analysis whereby to reduce the various phases of our experience to their determining factors in the sub-physical and sub-mental orders. Specific methods available for such a reductive analysis, though far from adequate, have been indicated already. Since greater progress has been made by the physical than by the psychological sciences toward the conception and detailed analysis of their limiting orders, the sub-physical and the sub-mental, respectively, greater results might be achieved in the beginning by employing physical objects (bodies and their motions), duly qualified, as positive and negative criteria for the sub-physical and sub-mental factors in our experience, respectively. But the other set of criteria should by no means be neglected, as all available methods will need to be utilized in the undeniably difficult investigations here projected; and such use will be essential to the perfection of the criteria in question, to the end of their greater serviceability for this general type of analysis.

Many suggestions and indeed more or less refined methods of investigation applicable in this undertaking would be uncovered by a canvass of the literature of physics, physiology and psychology, that deals in one way or another with psychophysical relationships. Although the investigations employing these methods have been based on assumptions respecting psychophysical relationships which must be reconstructed, there has nevertheless been some progress toward conceptions which are well grounded; and many of the methods employed in those investigations, as also the results attained by their use, would only require qualification in the light of sounder conceptions to supply materials and instruments for investigations of the type proposed.

Some of the main types of syntheses constituted by factors from the sub-physical and sub-mental orders may now be cited. These will be presented with a minimum of explanatory analysis, since, as it will be recalled, we are undertaking to offer only a provisional sketch of our doctrine and its general implications.

Sensory experience has already been considered at some length, and but little in addition need be said about it here. It is hardly necessary to point out that the range of our sensory experience serves to indicate specific capacities and limitations of our sub-mental equipment, as well as of the physiological organization associated therewith (sense organs, brain and nervous system, etc.). Those capacities seem to be

very limited, indeed, considering that our sensory experience supplies only a few sorts of indications as to the nature of the sub-physical order. And the data it does yield are difficult to assess as to their meanings respecting the sub-physical order, since, as our previous analysis has shown, numerous and complicated sub-physical factors within and without the organism are involved in sensation, so that it is extremely difficult to infer from sense data the nature of the various sub-physical entities contributing thereto. Nevertheless, refined methods of observation, experiment and analysis have made possible notable progress in this direction, particularly in regard to external sensations and the sub-physical entities therein. On the other hand, hardly a beginning has been made in identifying the sub-physical entities probably entering into motor and organic, but not capable of entering into external, sensations; and there seem to be well-nigh insuperable difficulties in the way of identifying such entities. Despite these limitations, it is only through an analysis of the sense data we do have that our knowledge of the sub-physical order is built up, and we must perforce be content with the knowledge thus made possible to us.

Perception involves all the factors, sub-physical and sub-mental, that enter into sensation, and additional factors from the two orders, not involved in sensation. Neural processes and motor attitudes are involved in perception, which are not called into play in sensation; and these differences indicate the operation of sub-physical factors in perception, additional to those involved in sensation. Perhaps the most significant differences between sensation and perception, however, indicate the operation of sub-mental factors in the latter, additional to those involved in the former. We refer to the manifold meanings which constitute an integral part of perceptions. The spatial localization of perceived objects, the suggestions of sensations other than those evoking the perception, the numerous concepts implicit in the percept, the memories, emotions, desires and anticipations aroused by it, the placing of it in the wider context of our knowledge and experience, generally, indicate something of the nature and range of the meanings which enter into our perceptions. While all these meanings have their sub-physical determinants, their more significant components must be referred to the sub-mental order, as a comparison thereof with bodies and their movements (our most reliable criterion of the sub-physical) clearly shows.

To obviate misunderstanding, we again interpose the reminder that factors enter into our experience, which are not properly classifiable under either of the two categories here under consideration. These include the organizatory factors involved in the development and main-

tenance of the organism and its activities on the purely physiological side; the logical and mathematical entities which in some sense enter into our experience; space and time (so far as these must be adjudged to possess a reality independent of other factors in experience); and values, ideals, standards of conduct, institutions, arts, tested knowledge and other controls of behavior which have been evolved in the course of human history. The functions of these various factors in human experience as well as their ontological status will be considered at a later point in the inquiry.

A very brief analysis of our affectional experiences (emotions and feelings) from the standpoint of the present conceptions may be presented. It is unnecessary for our purposes to consider the relations of the various types of affectional experience to one another; only their more significant components need concern us here. It is generally conceded that the dominating components of these experiences are motor or organic sensations of one sort or another. Glandular secretions and other bodily processes are also believed to be essentially involved therein. They are also commonly excited by external stimuli (leading to the exteroceptive sensations) and the meanings, inferences, judgments, valuations, ideals, etc., evoked by those sensations and the corresponding percepts. Sensations being the fundamental components of these affective states, much the same analysis applies to the latter as to external sensations and perceptions. They are initiated by and derive part of their qualities from sub-physical factors; and as in the case of external sensations and percepts, part of their qualities come from the sub-mental factors involved in the syntheses thereof. An important difference between them, however, lies in the fact that the stimuli to the sensational components of affective states emanate from *organized* physicochemical processes of the body. Also, as we pointed out in the previous chapter, the purely physical (our sub-physical) components of the stimuli for external and internal sensations, respectively, are probably of quite different sorts. They both agree, however, in being dependent on, and probably in deriving part of their qualities from, sense-receptor,[1] sensory nerve and central nervous system, all of which are structures peculiar to the organism, and have organized physicochemical processes correlated with them. These structures and processes, it need not be said, have their sub-physical components, though they are not wholly reducible, as we shall maintain, to terms of the latter.

External and internal sensations differ, again, as respects their rela-

[1] There do not appear, however, to be special receptors for all organic and motor sensations.

tions to a medium separating the source of the stimulus and the point of origin of the sensory impulse. Internal sensations do not, in fact, appear to be conditioned by a medium analogous to the media for visual and auditory sensations, unless the liquid plasma constituting the internal medium of the body generally (and which for any given animal species must be of quite specific composition) may be regarded as a medium in the former sense. If we do make this assumption, the sub-physical factors of the media for external and internal sensations, respectively, are obviously of qualitatively different kinds. Finally, the specific sub-mental components of internal and external sensations would, on the basis of the available evidence, be adjudged of different kinds, though integrated together in the sub-mental equipment as a whole.

For our purposes we may leave undecided the question whether emotions and feelings have as essential components cognitive and conative elements. In practice they are combined with such elements, and in adult experience at least their distinction therefrom, even for purposes of analysis, is hardly feasible. Indeed there seems to be a very close analogy between external sensations and perceptions, on the one hand, and internal sensations and emotions, on the other, since an emotion (or, still more, a complex sentiment), like a percept, seems to be a synthesis not only of sensational components but of meanings as well. The same thing is apparently true, to some extent, of the more definite types of feelings.

The last phase of experience to be considered in the present connection is that vaguely designated as the intellectual. The aim here, as with other phases of experience, is not to present a systematic analysis of our intellectual activity but only to point out some of the more significant relationships between sub-mental and sub-physical factors therein.

The discussion in the previous chapter showed, we believe, that cognitive processes embrace elements that are radically distinct from physical objects and events. Assuming that to be true, cognitive processes would be equally distinct, as regards these particular elements, from the sub-physical elements of physical objects and events. It was also shown that cognitive processes are to be distinguished, as regards these same distinctive elements, from sensations; and this fact affords additional evidence that those elements in cognition are distinct from sub-physical factors (at least those capable of entering into sensation) and probably also from the sub-mental factors in sensation, though doubtless combined with the latter in a unified whole of some sort. On the other hand, there are cognitive elements in our per-

ceptions and perhaps in our affectional experiences as well; these, however, are more in the nature of results yielded by past cognitive processes, than of cognitive processes reaching out, so to speak, for additional knowledge.

Intellectual processes, like all other phases of experience, are dependent in some sense on the nervous system and the body generally; and probably derive—we know not exactly how or in what measure—a share of their intrinsic characters from this physiological system, and hence from the sub-physical components of that system. The discussion of animal behavior in the previous chapter showed, however, that intellectual processes are not completely reducible to the physical (sub-physical) components of the nervous system or the body generally. We shall offer additional evidence for the same conclusion in a later chapter, where it will be shown that the thoughts, ideas and, in general, all the communicable products of cognitive processes are easily and in fact quite generally dissociated from the bodily processes concerned in their genesis, and that therefore they cannot be wholly identified with the latter.

As before considered, the thought products of cognitive processes are dependent for their communication and their social elaboration, on speech or other bodily activities, and on the use of symbols whereby such activities convey their intended meanings. The discussion in the previous chapter should suffice to show, however, that cognitive processes and products cannot be identified with the physical components of such reactions and symbols, since those processes and products were found to exert a type of influence on animal behavior that is distinctly non-physical in character.

Further evidence that thought cannot be identified with the symbols employed in its communication will be presented in a later chapter; it is there shown that thought is indifferent to the particular symbols whereby it is communicated, because innumerable systems of symbols and therefore infinitely diverse systems of physical phenomena may be employed in communicating the same thoughts; it is shown also that the particular symbols and sequences thereof employed in communication are dependent, for any given system of symbols, on the thoughts to be expressed, and not *vice versa*. This does not mean that the concepts implicit in a given language and its grammar do not have an influence on the cognitive processes to which such language is instrumental. These concepts, however, are not identical with the symbols whereby they are expressed. Nor is it implied that cognitive processes are not facilitated in varying degrees by the different systems of symbols which serve as their instruments, or by the arts

whereby symbols may be reproduced and transmitted from one place to another. It is clear from the previous considerations, however, that such symbols and the arts instrumental to their propagation are not identical with the thought conveyed by these means. The contribution of all such devices to intellectual activity lies essentially in their facilitation of communication, and the social elaboration of ideas thereby.

Finally, cognitive processes largely (though not entirely) have reference to bodies and their movements, and hence to the sub-physical components thereof. We must recognize here that cognitive processes in the concrete derive a large share of their intrinsic characters from the sub-physical order, since these processes cannot, in practice, be broken up into their sub-physical and other component elements. We must, in other words, recognize the contribution made to the nature of these processes, by both sub-physical and sub-mental elements (the sub-physical coming, of course, in the guise of the physical, which latter has its sub-mental elements as well). Again, however, that there are sub-mental elements in cognitive processes, which are radically distinct from any sort of sub-physical elements, is distinctly implied by the discussion in the previous chapter.

We may add, as further evidence for this conclusion, that the same *general types* of intellectual processes occur in human experience everywhere regardless of the sorts of physical objects and events by which the organism is environed. Class concepts, judgments, inferences, meanings and other cognitive processes will develop in conformity with the same general types regardless of the kinds of soil, topography, climate, and specific classes of objects to which they have reference. The same general types of processes also occur in societies with very unequally developed science, literature and other cultural factors. As we have recognized, however, the sub-mental factors in intellectual processes are specialized in relation to the environmental factors, physical and cultural, and derive a large part of their content from those factors.

So much by way of identifying the general types of sub-mental and sub-physical factors in our intellectual activities. The discussion of this topic suggests, if we mistake not, a special criterion whereby the specific rôles played in our experience by sub-physical and sub-mental factors may often be determined. This criterion is the relative indifference of given typical features of our experience to specific features of other general types that may be associated therewith. It is illustrated by the fact that cognitive processes and products are relatively indifferent to the particular symbols employed in their ex-

pression; and by the further fact that the same general types of cognitive processes arise wherever the given species (*e. g.,* the human species) has its environment, and whatever the stage of culture (if any at all) the given members of that species have attained. Also, to add an important detail, cognitive processes will be found guiding the behavior of the given animal to some extent, whatever its physical or cultural environment.

It could not be shown by this criterion, however, that the elements *common* to all environments do not have a determinative influence on psychical factors that operate in relation to all these environments. And it would not enable us to show that the nervous system and general bodily organization of a given species do not wholly determine that species' cognitive processes. To settle these questions, it is necessary to compare the reactions of animal organisms to the environment with reactions of inanimate bodies to similar environmental conditions.

These general applications of our hypothesis in the analysis of significant types of our experience will have sufficed, perhaps, to develop the hypothesis and its broader implications in such a way that it can be examined further, and refuted, corrected or elaborated as investigation may show to be justified. Our purpose has been to present the hypothesis in a provisional form only, not to make any elaborate tests of it. What we propose in presenting this hypothesis is the systematic analysis of all our experience on the basis of the conceptions set forth, insofar, that is, as scientific and philosophical interest may be concerned with the factorical analysis of human experience. This will mean that much of our philosophy and science must be reconstructed, if the foundations here offered shall be built upon. We have been concerned only to indicate the possibility, and perhaps the necessity, of a reconstruction along these lines.

It should perhaps be repeated again that in elaborating our hypothesis by way of interpreting sensations, perceptions, affectional experiences and intellectual processes from its standpoint, a systematic account of these phases of experience has not been intended. Nor is it implied that these phases of experience are more significant than other phases that might have been singled out for analysis. The hypothesis would certainly receive further elaborations if applied in the interpretation of these other phases of experience. Nor have we purported to offer a systematic account of sub-mental and sub-physical entities themselves. We have not undertaken, for example, to present a methodical conception of the organization of sub-mental factors in the individual, with their enduring elements, their complex interrelations,

their development in interaction with the environment, and so on; nor have we offered anything approaching an adequate concept of the sub-physical complex or thing, the interrelations among its various qualities, activities or parts, its interactions with other sub-physical complexes, or, finally, of its interactions with sub-mental factors. The discussion of these questions is hardly essential to our main purpose, for we have been primarily concerned to identify the sorts of entities that enter into animal (particularly human) behavior and experience; and, besides, these questions must largely wait, for their solution, on future scientific investigation and philosophical analysis, in which many investigators must take part.

Outline of a Theory of Human Experience

We must now indicate qualifications of the preceding analysis by allowing for the functions in experience, of a great variety of entities which are not properly, or at least readily, classifiable under any of the basic categories of entities so far considered. An excerpt from a recent treatise by Spaulding will serve to indicate the variety of entities for which allowance must be made in any factorial analysis of experience that aims to be at all comprehensive.

"Science and logic," says Spaulding, "have given us an almost inexhaustible list of entities only the most important of which can be indicated. For example, there are simple and complex, real and unreal, existent and subsistent, inorganic and organic, physical and mental entities; there are individuals, classes and series, things, events, qualities and relations; there are continuity and discontinuity, infinity, finiteness, and endlessness; numbers, space and spaces, and time; dimensions, correspondences, variables and constants; intensity, extensity, quantity, magnitude and measurement; unity and plurality, fields and domains, universes of discourse, the positive and the negative, conditions, connections and meanings. And one cannot neglect consciousness, sensations, judgments, reason, emotions, instincts, behavior, satisfaction, illusions, electrons, atoms, molecules, particles, forces, energies, directions, laws . . . There *are* the relations of identity, similarity and difference, inclusion, exclusion and contradiction, cause and function, dependence and independence, implication and consistency, whole and part, logical priority, symmetry and asymmetry, and transitivity and its lack.

"Finally, for religion and art, there are goodness and beauty, evil and ugliness, worth and its opposite; divinity, the supernatural, creation, emanation, immanence and transcendence, heaven and hell, God

and immortality, death and salvation. Be these errors or truths, inventions or discoveries, they must find their place in The One, if there be One, or must resist reduction, if there be Many.

"This rather long list of entities that in some sense are facts is merely illustrative of the tremendous manifold of 'things' which make up the *totality* of the universe. . . ." [1]

Verily, a formidable array of entities for which place must be found in a comprehensive theory of human experience! How shall we deal with it? Evidently nothing like a final theory could now be presented, considering that the experts on such matters disagree as to the nature of these various entities and of the various relations that subsist among them. We must confess to a special personal embarrassment in dealing with the matter, on account of a very imperfect acquaintance with recent developments in logic and mathematics, which are considered to have a vital bearing on matters of this sort.

Discretion is said to be the better part of valor and we propose to exhibit courage in this sense by retreating as gracefully as may be from the scene conjured up by this array of entities. Into the difficult questions, whether the entities of all these varieties ultimately constitute a One or a Many, which of them are existent and non-existent subsistents, respectively, and the like, we confess ourselves too discreet at this time to enter. Before taking our leave of the matter, however, we should like to indicate some very provisional views to which our puzzling over it has given rise. We would suggest that these entities (together with entities of other important varieties not mentioned by Spaulding) are, or are complex derivatives of, entities classifiable under the following heads:

(1) Sub-physical components of electrons, atoms, molecules, chemical compounds, colloids, energies, etc., with the interrelations of all these.

(2) Sub-mental entities, as manifested (together with sub-physical entities) in sensation, perception, affectional states, intellectual activities and other phases of experience, or, more generally, in the features or attributes whereby the behavior and experience of animal organisms are distinguished from other sorts of events.

(3) Organizatory factors indicated by the coördinated physiological activities of animal and plant organisms, and irreducible to terms of sub-physical entities. These factors are intimately bound up with sub-mental factors in the case of animal organisms at least, and may be fundamentally similar to those factors. In any case factors of

[1] *The New Rationalism,* pp. 434–435; Henry Holt and Company, publishers.

the two types are organically interrelated in the behavior and experience of animal organisms.

(4) Cultural factors, or the collective knowledge, beliefs, traditions ideals, institutions, standards, arts, customs, etc., arising from the social life of human beings and, to a far lesser extent, of some other animal species. Entities of this category, taken together, are complex derivatives of entities of all other categories, but their *genesis* depends primarily on sub-mental factors, particularly on the factors of this category which are most active in cognitive processes.

(5) Space.

(6) Time.

(7) Logical entities.

(8) Mathematical entities.

There may be other classes of entities in the universe, but there seems to be *adequate* evidence for the subsistence of entities of these classes only, together with their complex derivatives. It may perhaps be added that there is no adequate evidence *against* the subsistence of other entities believed in by many competent thinkers, such as a God, an Absolute, disembodied souls, etc. We should be willing to admit the possibility and even the probability that there are many classes of entities in the universe of which we have not the slightest intimation, since the human mind is characterized by most drastic limitations, and may not be capable of apprehending more than a few sorts of the entities in the universe.

A detailed elaboration of the theory just outlined is clearly out of the question, in a work of this character. Only for the first four categories of entities do we hold ourselves specially responsible, as to their bearing on our particular problems. For the remaining four we limit our responsibility to a recognition of the part which they play in human experience and, more generally, in vital phenomena; and to the provisional assumption that they are, in principle, respectively independent of all other categories, while entering with them in infinitely diverse types of complex syntheses having a place of some sort in reality. We shall present only some of the grounds that appeal to us personally (and these of a very fragmentary character) for provisionally assuming that entities of these several categories are distinct from one another; and only a few illustrations in support of the hypothesis that all entities are, or are complex derivatives of, entities coming within one or more of these eight categories.

The evidence we have to adduce in support of the proposition that entities of the first four classes are severally distinct from entities of other classes is set forth at length in the immediately preceding, the

present, and the next five chapters. Qualifications of this assumed independence have been indicated with respect to Class (3) in its relation to Class (2); and with respect to Class (4) in its relation to all other classes. Qualifications of the assumed independence of other categories will be indicated as we proceed. It is not implied, of course, that the independence of any category is absolute in the sense of the incapacity of its members to enter into complex syntheses with entities of the other categories. The independence meant is that of ontological uniqueness, or radical dissimilarity to entities of other categories.

What we have to say about space will be presented first. Space does not appear to depend on sub-physical or sub-mental entities or on the synthesis of both sorts of entities into physical objects or mental processes. Space as such seems to be, partly at least, in the nature of an empty container which is perfectly indifferent to the contents that may fill it. As a matter of fact, the contents of space are in constant circulation, and a given space may be at different times more or less empty, perhaps quite empty altogether. The pressure of our practical experience has forced on us the belief, which can hardly be explained away, that space is of this general character.

We should say, however, that pure space is always synthetized in our experience, with time, sub-physical and sub-mental entities, and perhaps with entities of other types. It cannot for this reason be apprehended in its pure form. We *infer* the existence or subsistence of such an entity, just as we infer the existence of sub-physical and sub-mental entities; the conclusion that such entities exist or subsist is necessary to account for the variety and order of our experiences. We suppose it is the synthesis of space with other entities in our experience that gives rise to relativistic conceptions of space and to the denial of space in an "absolute" sense. From the standpoint of experience as given, space is relative; it is only by a factorial analysis (*analysis in situ*) of experience that the assumption of a pure space can be justified.

Space seems necessary to the existence of sub-physical entities, since extension and motion are essential properties of these entities. *Perhaps,* however, sub-physical entities could, by a further reductive analysis, be resolved into spatial attributes and component elements which are not spatial. In fact that which *constitutes* a physical existent or sub-physical entity as different from the space it occupies is non-spatial in character, though occupying and acting in space.

We may omit from consideration questions pertaining to the on-

tological dependence or independence on space, of entities of other categories. They all apparently enter into syntheses that have some sort of spatial localization, but none of them appear to possess spatial attributes in the same sense that sub-physical entities do. At least they would all possess, even as do entities of the latter type, elements which are quite irreducible to space.

Much the same sort of considerations would enter into the analysis of time. It seems, like space, to be quite indifferent to the events—physical, mental, etc.—that are in time, and we suspect that in itself it is indifferent to all these events put together. Practically we regard time somewhat as we do space, that is, as being potentially empty or partially so, or at least as allowing a measure of control over its contents; though, as in the case of space, the events that inevitably occur must be in some section of time (if we may so put it): they cannot be eliminated from it altogether.

All other types of entities enter into syntheses which are in time, in somewhat the same way as into syntheses localized spatially; but they all possess elements which are not reducible to time. Again, time, like space, is relativistic, considered from the standpoint of our experience; but the exhaustive analysis of experience into its constituent elements justifies and necessitates the conclusion that one of these elements is a pure time distinct from other classes of entities.

We shall not take it upon ourselves to define exactly the intension of the category termed logical entities. We would only say that there are *some* entities properly designated logical, being irreducible to terms of entities coming within any other categories. Of such are the principles of identity, contradiction and excluded middle, the subsistence of which would not appear to depend on anything else in the universe. Whether or to what extent terms, propositions and concepts are independent logical entities we shall not undertake to say; we are disposed to think, however, that terms, propositions and concepts referring to entities of other classes are not exclusively logical in character. The same remark would perhaps apply to such relations as implication and consistency, similarity and difference, inclusion and exclusion, whole and part, symmetry and asymmetry, dependence and independence, and the like.

Logical entities enter into relations of various sorts with entities of other kinds. The logical analysis of sub-physical or any other type of entities involves such relations, as does also the investigation of logical entities and principles on their own account. The part played by these types of intellectual activity in science and philosophy indicates that logical entities vitally influence human behavior and experience.

If logical entities are involved in cognitive processes generally, as seems not unlikely, then their influence may be as widely distributed as is life itself. But, though dependent on cognitive processes for their apprehension and practical influence, some logical entities are apparently capable of being presented in experience in a pure form, or nearly so, being in this unlike entities of other categories, with the possible exception of mathematical entities.

We must be quite as tentative in speaking of mathematical entities. Purely mathematical entities appear to be largely if not entirely limited to numbers, their classes, series and relations; though the science of numbers is or may be applied in the investigation of entities of all other classes, as well as complex derivatives thereof. Number seems to characterize entities of other categories, since these in varying degrees are distinct from one another, can to some extent be counted, enter into manifold relations characterized by these same attributes, and so on. In other words, entities and their relations are numerable, enter into series susceptible of quantitative analysis, group themselves into numerically distinct classes, etc. Mathematical entities therefore enter in various ways into complex syntheses with logical entities, space and time, sub-mental and sub-physical entities, culture and organizatory factors. Numbers, classes, series and relations thereof may apparently subsist, however, in complete independence of any other entities; and are perhaps capable of being presented thus, or approximately so, in human experience.

We shall take our leave of the four categories just discussed—space, time, logical and mathematical entities—with these admittedly fragmentary and perhaps largely erroneous considerations; and with the reminder that we are not holding ourselves specially responsible for them, but only indicating, in the most tentative fashion, what seems to be the nature of their contributions to human experience. The four categories of entities for which we acknowledge a larger measure of responsibility—sub-physical and sub-mental entities, culture and organizatory factors—are analyzed at some length in this and other chapters, and that analysis may be consulted for the results thereof that bear specially on the present problem.

Now, our hypothesis is that all varieties of entities coming within the range of our knowledge are reducible to entities of the eight categories specified, or to complex syntheses of entities coming within two or more of these categories. Only a few varieties of complex entities will be discussed by way of illustrating and justifying this hypothesis. A definite appraisal of the hypothesis would of course entail the examination of other varieties of such entities not here considered.

The hypothesis has already been elaborated to some extent in our attempt to define the various categories of entities and their relations to one another. Further cases will be drawn from classes of entities whose ontological status is much debated in the current literature and which come more nearly than do other classes of entities within the range of our own special investigations. But an exhaustive analysis of these cases will not be undertaken. And we concede in advance the obviousness of much that will be said regarding these cases, and the possible erroneousness of much of the remainder.

A detailed factorial analysis of sensory qualities has already been presented, indicating the nature of the contributions made to these qualities by sub-mental entities, sub-physical entities of various sorts, and organizatory factors involved in the conditioning physiological processes. Other phases of experience have also been analyzed from the standpoint of sub-mental and sub-physical elements therein.

A factorial analysis of the organism as a whole, as herein presented, shows that all organisms, besides being characterized by spatial and temporal attributes, embrace sub-physical and organizatory factors; that animal organisms include, in addition, sub-mental factors; and that developed human organisms, and perhaps other classes of animals as well, also embrace cultural factors and, in some sense, logical and mathematical entities.

Culture itself, taken as a whole, refers to and is partially constituted by entities of all other categories. Culture arises, however, only where and when intellectual processes of a relatively high order occur, though these have as objects entities and complexes thereof which, so to speak, are accessible to the intellect. And intellectual processes, as already shown, are complex derivatives of entities coming within sub-physical, sub-mental and organizatory categories; and they also generally embrace (especially on their more subjective side) cultural factors, logical entities and mathematical entities.

As already intimated, concepts, terms and propositions referring to other than logical entities are complexes of entities from two or more categories, assuming that logical entities are involved in those complexes. The same remark covers or could be extended to universals, abstract qualities, laws of nature (in the broadest sense of the term), principles, theories, hypotheses, truths, errors, etc., not pertaining to logical entities. All these have at least part of their being in the particular concrete objects, however simple or complex, to which they refer, and hence in the several sorts of entities constituting those objects. We should say also that most if not all complex entities of these types embrace sub-mental and cultural components, vary-

ing, as they do, from one mind to another, and from one stage of culture to another.

Memories, images, dreams, illusions and hallucinations—assuming them all to have a sensuously characterized content—would on our analysis embrace components from at least the sub-physical, sub-mental and organizatory categories of factors, probably also from the category of cultural factors, and, in particular cases, from other categories as well. All such complexes would perhaps be characterized by spatial and temporal attributes also.

Error would appear to involve, in all its instances, sub-mental factors, sub-physical factors (for the latter are components of the organism, even when the given error does not refer to factors of this type) and organizatory factors (which are also essential components of the organism). Errors also often if not generally involve logical and mathematical entities, while particular classes of errors refer to entities of these categories. A similar statement would hold for the relation of error to space and time. Innumerable errors are of course embodied in religious, philosophical, scientific, political, economic and all other sorts of traditions; and these are propagated through educational processes, and largely determine later errors, or the same errors in a modified form.

Much error is due, of course, to native imperfections of our sub-mental equipment. Many problems which we attempt to cope with are apparently too difficult for our minds to solve, even with unlimited time and the best possible sort of educational discipline. Other errors, due in a sense to the same cause, are exposed through continued investigation of their subject-matter, and may, through educational processes, be eliminated from the minds of people generally. Differences in native intellectual (sub-mental) equipment, training and experience (the latter two being complexes of many different types of entities) account for more errors arising in the cognitive experience of some people than of others.

The part played by cultural factors in error has been suggested already. Errors embodied in tradition are due to much the same sorts of factors as those already indicated—imperfections of native intellectual equipment, limitations of training and experience, defects of the sub-physical and organizatory components of the organism, errors and other imperfections of prior traditions, etc. Unusual conjunctions of inorganic and organic (including human) events have of course been a potent source of error throughout man's history.

Expanding the latter point, we may say that potent sources of error to-day, as always, are unusual or unfamiliar conjunctions of events

in the environment, both organic and inorganic. The unusual achievements of great personalities have of course given rise to errors respecting those personalities, their traits and the sources of their powers. These things lead to error, of course, only in conjunction with imperfect cognitive processes—hasty inference, uncritical acceptance of other people's statements, and the like. Such errors are also in large part due to erroneous traditions, limited training and experience, etc., themselves due to various types of factors previously specified.

Truth conceived as the opposite of error would be accounted for in similar terms, with of course the various entities therein yielding results the opposite of error. Truth in its other meanings we cannot here consider. Perhaps many if not most truths, *e. g.,* the truth of the principles of identity and contradiction, have some sort of reality apart from sub-mental factors (which are involved in error and truth in the senses previously considered). According to the conception of truth as reality, particular truths would be constituted by the particular entities or complexes thereof which those truths mean or involve.

Our final instances shall be beauty and goodness. Beauty in its various forms embraces or involves sensory qualities, their concretions and extensions, feelings, emotions and sentiments, desires, ideals and aspirations, inferences, judgments and meanings. Our previous analysis of these classes of components should have made it clear that entities from all our basic categories enter therein. Sub-mental, sub-physical and cultural entities would appear to be the most significant components of the aesthetic experience, though all other sorts of entities are involved therein in one way or another. The sorts of entities specified as the more significant would of course be illustrated by the various sorts of beauty associated with land and water areas characterized by different colorations, contours, flora, fauna and the like; by the individual quality of the aesthetic experience, varying, as it does, from one artist to another, and from one appreciator to another; and by differences in aesthetic taste correlated with dissimilar cultural backgrounds, as instanced by the contrasts between mediaeval and renaissance art, Japanese and European, etc. Perhaps the sorts of entities always involved in the aesthetic experience, including the most primitive types, are the sub-mental, the sub-physical, the organizatory and of course the spatial and temporal, these at least being essential to the experience of animals for which beauty can be.

A similar analysis would apply to goodness in the ethical sense of the term and to ethical realities generally. None of these have any meaning or existence apart from the behavior and experience of animal

organisms, perhaps of human beings alone. Human conduct ethically regarded pertains of course not to human beings alone, but also to other forms of sentient life and even to inanimate objects. All phases of human experience have their ethical aspects, and hence all entities coming within the range of our experience bears in one way or another on ethical relations. It is obvious, of course, that ethical realities depend in a special sense on sub-mental factors, and on ethical traditions growing out of human experience in the past.

In presenting this factorial analysis of human experience or, in other words, of various sorts of realities somehow given in experience, it is not implied that the complex syntheses of entities in our experience are in any sense less real than the component entities themselves. In a sense such syntheses are more real, by having more sorts of reality given therein. A chemical compound, an organism, a culture or an aesthetic experience is not the less real for being, not one sort of entities, but a number of sorts synthetized together in a unified way. A reductive analysis does not analyze away, but on the contrary, serves to reveal the variety, the richness, the order of reality of that which is analyzed.

CHAPTER IX

THE INCOMMENSURABILITY OF PHYSIOLOGICAL AND THE CONCOMITANT PHYSICOCHEMICAL PROCESSES

In previous chapters the principal types of the mechanistic hypothesis have been presented, and the corresponding interpretations of mental processes critically examined. A factorial analysis of the physical and the mental was also presented, showing that both these are complex syntheses of entities of various categories, including sub-physical and sub-mental entities. It was shown, further, by a comparison of sensory elements embraced by organisms and inorganic bodies, respectively, that the former include sensory elements of types not entering into the composition of the latter; and that this fact implies that the organism on its physiological side embraces physical, or non-physical, or both physical and non-physical elements not found in inanimate bodies. It was also pointed out that current conceptions of matter and energy are incomplete, because they allow for certain classes of sensory experience only, and ignore other classes presumably offering valuable data as to the nature of the physical world. All these conclusions were supported by an analysis of the evidence bearing on the problems at issue.

Physicochemical interpretations of purely physiological processes (those not obviously involved in the experience and overt activity of the organism) have not so far been critically examined, but only presented and compared, logically, with vitalistic interpretations of these processes. The exigencies of the previous analysis obliged us, however, to anticipate the outcome of our examination of mechanistic hypotheses in their physiological applications, by assuming that organizing factors, distinct from the correlative physicochemical conditions, are responsible for the specific qualitative, quantitative, spatial and temporal relationships represented by the physiological organization of those conditions.

That assumption is now to be justified by an analysis of physicochemical interpretations of physiological activity. It will not be necessary in this analysis to consider separately the various types of the physicochemical hypothesis, because they all purport to account for physiological activity in terms of physical and chemical processes

alone, though stressing somewhat differently specific features of this activity or specific types of physicochemical processes entering therein; and we are undertaking to show that *no* type of physicochemical action nor any combination of such types will account for the distinctive features of physiological activity. Nor will it be desirable, in this discussion, to reduce the physical factors in physiological processes to terms of their sub-physical and sub-mental components, since, as we have recognized, there is a detailed one-to-one correspondence bebetween sub-physical entities and the physical objects and events through which they are made known to us; and nothing would be gained for our present purposes, therefore, by introducing into the discussion the analysis of physicochemical processes into elemental components of the types indicated.

In essaying the task before us it is unnecessary to minimize the service rendered the biological sciences by mechanistic conceptions of life. As Haldane has shown so well, mechanistic hypotheses respecting physiological phenomena have yielded and continue to yield results of the greatest value for an understanding of those phenomena, whereas vitalistic hypotheses of the type hitherto current have not yielded any substantial results, but at most only registered the failure of mechanistic hypotheses to do all that was expected of them.[1] Yet these successes of physicochemical hypotheses prove only that they have heuristic value, not that the monistic physicochemical interpretation of life is an ultimately valid one. There is really no experimental evidence that physiological processes are identical with the physical and chemical conditions on which they depend:[2] "we cannot tell what exactly becomes of the atoms and molecules which pass into the body," for "we must not mistake measurements of the balance of matter or energy entering and leaving the body, for information as to the manner in which this stream passes through the living tissues";[3] in short, "the application to physiology of new physical and chemical methods and discoveries, and the work of generations of highly-trained investigators, have resulted in a vast increase of physiological knowledge, but have also shown with ever-increasing clearness that physicochemical explanations of elementary physiological processes are as remote as at any time in the past, and that they seem to physiologists of the present time far more remote than they appeared at the middle of last century."[4]

[1] *Mechanism, Life and Personality,* Lectures I and II.
[2] *Ibid.,* p. 34.
[3] *Ibid.,* p. 36.
[4] *Ibid.,* p. 47.

Not only are mechanistic hypotheses unproved, but a great many significant facts cannot be reconciled with any such hypothesis. By the mechanistic theory of heredity, for example, "we have to account, not for only one, but for a large number of stupendously complex reproductive mechanisms within the original germ-plasm, and for their endless division and multiplication. The real difficulty for the mechanistic theory is that we are forced, on the one hand, to postulate that the germ-plasm is a mechanism of enormous complexity and definiteness, and, on the other, that this mechanism, in spite of its absolute definiteness and complexity, can divide and combine with other similar mechanisms, and can do so to an absolutely indefinite extent without alteration of its structure. On the one hand we have to postulate absolute definiteness of structure, and on the other absolute indefiniteness. . . . The mechanistic theory of heredity is not merely unproven, it is impossible. It involves such absurdities that no intelligent person who has thoroughly realized its meaning and implications can continue to hold it." [1] The breakdown of the mechanistic hypothesis in its application to heredity invalidates it for biology generally, since "our conception of heredity involves every part of biology; and if we cannot frame a mechanistic theory of heredity we are equally at a loss in connection with the ordinary phenomena of metabolism, and we have no right to use mechanistic hypotheses in connection with these phenomena. We have also seen already that the ascertained facts do not in any case point to mechanistic theories of the ordinary activities with which biology deals." [2]

Not only have mechanistic hypotheses failed hitherto to account for the distinctive features of physiological phenomena, but the radical differences between physiological and purely physicochemical processes warrant the conclusion that the two types of processes are quite incommensurable. The justification of this proposition is the task to which we now turn.

The living organism is the scene of a vast interplay of material processes or, in terms of elementary physical conceptions, of masses and their motions. The processes falling under this general category include a wide range of phenomena termed chemical, that is, chemical reactions of certain types, coördinated in complex series or systems. Digestion, metabolism and locomotion represent or depend on such complex series of chemical reactions. Changes in the forms and distribution of the energy associated with the chemical substances in these systems always accompany the interaction of such substances.

[1] *Ibid.*, p. 58.
[2] *Ibid.*, p. 60.

Then there are many distinct types of physical action more or less closely correlated with, but somewhat distinct from, the chemical reactions. These include simple cohesions and adhesions, filtration, osmosis, diffusion, electrolysis, electrical currents, gravitational pressures and tensions, the mechanical combination of cells, tissues and other component structures of the body, the molar motions of body parts and of the body as a whole, the transference of materials from one part of the body to another, etc.

The Chemical Aspects of Physiological Activity

Let us consider first some of the more important types of chemical action in the living organism, restricting the analysis to phenomena of the type dealt with by classical chemistry, and then introduce such qualifications of the analysis as may be necessitated by a consideration of colloidal phenomena and the various types of physical action found in the organism.

What is the most significant feature of bio-chemistry, considered from the standpoint of physiological activity as a whole? Undoubtedly it is what may be variously termed the system, coördination, organization, regulation or equilibrium of chemical reactions occurring in various parts of the body. Within narrow limits of variability, the interacting chemical substances in the body must be present at the right times and places, and in the right sorts and amounts, if the equilibrium of the system is to be maintained.

In digestion, for example, the food substances must be transported from part to part of the digestive tract, digestive fluids must be secreted in strict correlation with this movement, and in the requisite qualities and amounts, a number of specific enzyms must be produced and transported to the proper places and in the proper amounts for the purpose of regulating those secretions, the various end products prepared by these means must be absorbed in very specific ways from the small intestine, and transported to all the tissues of the body for use therein, or temporarily stored up in the liver, muscles or other tissues for future use. During all these processes, the stomach, intestinal walls and certain of the secreting glands must be protected against being themselves digested by the proteolytic enzyms produced by those glands, protection which is provided, in the opinion of some biochemists, by anti-enzyms of the requisite chemical constitution. The requirements of this system are obviously very exacting, since any considerable disturbance of its chemical equilibrium must result in the speedy death of the organism.

Even more striking correlations of chemical action in the body are effected by means of numerous specific hormones. A hormone secreted by the hypophysis affects the growth of the organism; secretions of the thyroid regulate the development of the nervous system, the growth of connective tissue and metabolic processes generally; the thyroid itself is stimulated, in certain emergencies, to augmented activity through adrenin, a hormone secreted at a distant part of the body, etc. These and other types of chemical correlation through hormone action signify a most delicate balance of the chemical reactions involved, since, as is well known, serious disturbance in this balance, as by the failure of a given gland to secrete its hormone at the right time, or to stop secretion at the right time, will throw the whole system out of gear, and may result in the speedy death of the organism.

It is unnecessary to multiply instances, for the chemical activity of the organism is replete with them.[1] We shall only observe further that the system is by no means a purely automatic one, but is characterized by a highly developed capacity for regulating itself in various ways, as the disease-resisting functions of the blood, adaptive behavior with respect to external stimuli, the orderly change in chemical reactions according to different functional demands of the organism, and many other phenomena demonstrate.

Let us note, finally, that chemical action is involved in all the developmental processes of the organism, so that structures, such as connective tissue and the bony skeleton, whose functions are not primarily chemical in character, are all built up and maintained, in part, through chemical action. Needless to say, all these regulative and developmental processes, like the recurrent phases of physiological activity, depend on the requisite types of chemical action occurring at the right times and places and in the right amounts, with temporal changes in these types of action occurring as required. And the *maintenance* of every bodily structure at the requisite level of functional efficiency is equally exacting as regards the chemical actions involved therein.

Can such a complex system of coördinations be accounted for in terms of the chemical and associated physical actions that are coördinated?

We would say that, viewed in relation to the structure and orderly activities of the organism as a whole, specific chemical reactions are in the nature of *chance* occurrences, since each chemical body is in itself an independent entity which can interact with similar entities

[1] For further examples of specific correlations of physical and chemical processes in the body, see Henderson, L. J., *The Order of Nature,* pp. 80–86.

only by being brought into juxtaposition with them under the requisite physical conditions. Such independent entities hardly bring themselves together in the innumerable specific ways requisite to the development, maintenance and orderly activity of the organism. How, if we may so put it, should the anarchical, individualistic tendencies of discrete molecules of matter be converted into the highly organized, coöperative action of the living organism, and *through* the operation of just those same anarchical, individualistic tendencies? The proposition that such is the case appears utterly untenable, when its logical implications are duly considered, and without support from any of the analogies that might be drawn from our experience in other fields. Let us elaborate this argument somewhat further.

The interaction of diverse chemical substances yields only other chemical substances (simple or compound), together with transformations in the associated energies. However extended and complex such a series of interactions might be, the end products thereof would be nothing but a number of specific chemical substances, together with a redistribution of energy among the various forms entering into the series, and in different quantitative relationships with the associated chemical bodies. More complex substances are produced in the living organism than elsewhere, of course, and transformation of energy is proceeding therein all the time, but the *sole* products of physicochemical action within the body are chemical substances and transformations (including the absorption and liberation) of energy. The actions of the chemical compounds so produced, with their associated energies, remain *chance* actions to the end of the story, considered with reference to any organization other than molecular (and perhaps colloidal) structures. And all the juxtapositions of chemical bodies (with their associated energies) at the right times and places and in the right qualities and amounts, which are requisite to the production of other chemical bodies of different formulae and to the specific transformations of energy essential to the maintenance of the organism, cannot be accounted for in terms of physical and chemical action itself, for all those juxtapositions taken together are precisely those requisite to vital structure and activity, and are *coördinated* together in the most specific fashion—something not explicable by the *chance* actions of the infinite number of chemical bodies involved. *Structure and organizing capacity* simply have to be taken for granted by the physicochemicalists, if they are to be explicit with regard to these features of the organism.

The physicochemicalists might rebut this line of argument by the contention that chemical reactions in the body always depend on the

given physical conditions, that the reactions themselves are of the given chemical substances, and that the progress already made in physical and biological chemistry fully justifies the conclusion that all the chemical reactions in the body can be eventually accounted for in terms of the chemical substances involved, together with the accompanying physical conditions.

We could, by way of rejoinder, expatiate on the point made many times before, that the physicochemicalists are restricted by their methods to the discovery only of physical and chemical actions in the body, and that, so long as they adhere exclusively to these methods, they can never hope to discover anything else, even if there be such. They simply are *blind* to any such possibility. We may say, with Johnstone, "that the mechanistic conception of life is only the result of the extension to biology of *methods* of investigation, and not a legitimate conclusion from their *results*."[1] But this by no means disposes of the mechanistic hypothesis, and we must examine well the defense put up by advocates of the hypothesis against criticisms such as ours. Their argument would be that every type of chemical action necessary to the orderly development and activity of the organism can be accounted for in terms of the chemical bodies involved, plus the attendant physical conditions; that these bodies themselves, their chemical constitutions, their quantities, their locations and the attending physical conditions can all be accounted for in terms of antecedent or concomitant physical and chemical processes, and so on to the beginning of the whole series; and that, consequently, there is no necessity for assuming the intervention in the series, of factors not physicochemical in character.

It cannot be denied that the purely chemical changes involved in any system of functional activity are continuous, and that particular chemical reactions therein are partially determined by preceding reactions and partially determinative of succeeding reactions. But this fact hardly touches the question at issue between the mechanists and their opponents. The latter willingly concede the continuity of chemical changes in the organism, but claim that physiological processes have as additional determinants factors which are not physical or chemical in character, and that these are also continuous in their action. In other words, according to this view, physiological processes are complex syntheses of entities belonging to at least two radically distinct categories, namely, the physicochemical and the organizatory.[2]

[1] Johnstone, James, *The Philosophy of Biology*, p. 121.

[2] The latter category is designated in various ways, of course, and the current conceptions thereof also differ more or less from one another.

Now, this hypothesis cannot be tested by the experimental isolation of the physicochemical factors in physiological processes from the organizatory factors assumed by advocates of the hypothesis to be also essential determinants of those processes, since the isolation of the physicochemical factors would, according to this hypothesis, destroy the given physiological processes; and, even were such isolation possible, the organizatory factors would not, according to the same hypothesis, come within the range of our perceptual powers, however real those factors might be.

A different type of method is therefore necessary for the testing of this hypothesis. Only two specific methods appear to be available for this purpose. One of these is the method of concomitant variations. Results yielded by this method, as applied to the present and other groups of problems, are reported in a later chapter. These show, as regards the problems here under consideration, that correlations between chemical processes in organisms, on the one hand, and the morphological characteristics represented by physiological processes, on the other hand, are of such an order as to afford little or no support to physicochemical hypotheses and, indeed, rather definitely to refute these hypotheses in certain of their applications.

The other method is *analysis in situ,* applied to the present problems by comparing the physical and chemical phenomena pertaining to organisms and inorganic substances, respectively. This method was applied in the previous analysis and yielded the results there indicated. The significance of these results might be made more clear, however, by presenting them in a somewhat different form.

Chemical reactions in the inorganic are dependent on the kinds and amounts of chemical bodies brought together at the given time, as well as on the attendant physical conditions (temperature, moisture, pressure, etc.). Such reactions do not have reference to chemical reactions and physical conditions that are spatially and temporally remote from themselves; or, more accurately, the given chemical reactions do not have any reference to reactions and conditions that are not immediately related, in a physicochemical sense, with themselves. There is such reference in the case of living organisms. Chemical reactions in widely distant parts of the body are interrelated in very specific ways; and reactions at any given time are similarly related to later reactions. Our contention is that such relations are not in, or a part of, or derived from, the chemical reactions themselves. The chemist and the physicist are not concerned with relationships between the phenomena observed by them and similar phenomena

spatially and temporally remote therefrom, and not related with the given phenomena in a physical or chemical sense. The physiologist is concerned primarily with such relationships. In other words, physics and chemistry are not concerned with morphological problems at all, and for the reason that such problems cannot be attacked by their methods; whereas physiology and other branches of biology are interested in these problems, and for the reason that they are *the* problems of the organism which constitute the *raison d'être* of the biological sciences.

It may be pointed out that the conclusions yielded by our analysis of physiological processes apply also to the *immediately contiguous* stages in typical series of chemical changes in the organism, where, as is normally the case, the end products of one stage find waiting for them, so to speak, suitable chemical substances (and physical conditions) yielded by some stage of *another* series, so that the next stage of the given series can occur. There are, throughout, very definite spatial and temporal relationships among the purely physical and chemical processes of the organism, and these relationships constitute into a unified whole the many series of physicochemical processes which, as such, are independent of one another.

A similar analysis applies to the relationships among the *kinds* and *quantities* of chemical substances and physical processes in the organic and inorganic respectively. Nowhere in the inorganic are there the relatively constant, specific relationships between unlike substances and processes, and between the amounts thereof, that characterize all living organisms. The solar system does not present an exception, for the relations therein are between bodies that are homogeneous, so far as they enter into *this* system. The dissimilarity of the two cases would be still further emphasized by a comparison of their spatio-temporal relationships, including variations in these relationships. Somewhat similar observations would apply to crystalline structures. All the relationships in these and other inorganic systems can be interpreted in terms of matter and energy, whereas this seems quite impossible in the case of vital systems.

The qualitative, quantitative, spatial and temporal relationships entering into the economy of the organism, together with *regulatory* variations in these relationships necessitated by environmental influences and perhaps also by hereditary peculiarities of individual organisms, would therefore appear to distinguish the organism, and in a radical sense, from any type of phenomena found in the inorganic. In short, the vital organization of physical and chemical processes

is not in those processes themselves, since these do not, as such, embrace the specific qualitative, quantitative, temporal and spatial relationships that characterize all organization of this type.

Physical Processes in Physiological Activity

The qualifications of the foregoing analysis necessitated by a consideration of physical processes and colloidal behavior occurring in the organism may be briefly indicated. Reserving for later consideration the rôle played by colloids in the activity of the organism, we may say that the physical changes occurring in the body—osmosis, diffusion, electrical phenomena, molar motions, etc.—represent, in so far as they are to be distinguished from chemical reactions, an enormous number of discrete processes, which must be qualitatively, quantitatively, spatially and temporally specific, if the developmental and functional requirements of the organism are to be fulfilled. Within narrow limits, these actions must occur in the right sorts and the right amounts and at the right times and places if the genesis, development and maintenance of the organism are to be accomplished. These processes are of course coextensive with the chemical reactions in living organisms, and the coördinations thereof, however effected, must be as extensive and specific. The mechanistic interpretation of such coördinations is subject to the same criticisms as the corresponding interpretation of chemical correlations. This interpretation implies in either case that one type of process (the physical or chemical) can produce another type of process (the organizatory or equilibrative) which is radically unlike the former, and which chemical and physical analysis really cannot discover or investigate. Coördination, equilibration, organization of these processes, physical or chemical, in conformity with the exacting requirements imposed by every living organism, simply is not in those specific processes. An *analysis in situ* would demonstrate this in detail for the physical as for the chemical processes of the organism.

These criticisms would be still further emphasized by considering the organization of *both* the chemical and the physical actions in the organism, for the two types of action are not separated, but intricately combined together in the adaptive, regulative fashion before characterized. It is of course only through the combination of innumerable physical and chemical actions, of very diverse types, and, as already emphasized, according to quite rigorous quantitative, qualitative, temporal and spatial specifications, that the requirements of the or-

ganism can be fulfilled. If our analysis demonstrates the untenability of mechanistic hypotheses as applied to physical and chemical processes considered separately, it should serve to emphasize the falsity of those hypotheses when applied to the complex syntheses, in the living organism, of both types of processes.

Colloidal Behavior and Physiological Activity

It is now common to speak of life as the chemistry of the colloids, owing to the attention which colloidal chemistry, especially of the proteins, has latterly attracted, just as, earlier, life was spoken of as the chemistry of the enzyms or the chemistry of phosphorus, when the functions of those substances in the body were being discovered.[1] Let us see, so far as one who is not a specialist in this field may be permitted to discuss the problem, if there are no difficulties in the interpretation of life in terms of colloidal phenomena.

Colloids, and especially the proteins, are characterized by their state of aggregation, making it impossible for them to diffuse through parchment or other membranes; by the marked and often irreversible changes undergone by colloidal solutions upon the addition of varying but generally small quantities of electrolytes; by the reversibility (of organic emulsoids), under the requisite physical conditions, from "gel" to "sol," and *vice versa;* by the low osmotic pressure of substances in the colloidal state (and derived properties), etc. We may note, as general class characteristics of these phenomena, that the colloidal state is a special case of disperse systems, in which two phases (or substances) differing in one or more physical properties have a large surface of contact, or interface; and that all substances can be brought by suitable methods into the colloidal state—or at least the possibility of so doing is generally recognized. Two less general but quite significant characteristics may be noted: One is the marked similarity in colloidal characteristics of substances differing profoundly in chemical constitution;[2] the other is the dependence of all the properties of a gelatin sol, and to a marked degree, on the previous treatment and history of the sol.[3]

There is controversy as to the type of the chemical action associated with the colloidal state, some colloid chemists maintaining that such

[1] Johnstone, James, *The Philosophy of Biology*, p. 108.

[2] Hatschek, Emil, *An Introduction to the Physics and Chemistry of Colloids*, fourth ed., p. 81.

[3] *Ibid.*, pp. 82–83.

action exhibits fundamental differences from the chemical reactions of crystalloidal solutions, others insisting that this is not the case. Moore may be taken as a representative of the first view,[1] while Loeb is a representative of the second.[2] Moore maintains that molecules completely saturated atomically form colloids bound together by affinities between the constituent molecules, that such colloids are characterized by their lability, mobility and weakness, considered as chemical bodies, and that the colloidal condition, thus characterized, is that of a balance or play of energies in the most delicate equilibrium, though colloids of great stability are known.

Loeb maintains, on the contrary, that the chemical behavior of proteins (to which he restricts his discussion in the work cited) conforms to the laws of classical chemistry, and that "the chief if not all the characteristics of colloidal behavior can be explained mathematically from the difference in diffusibility between colloids and crystalloids, while the tendency of the protein molecules to form aggregates plays only an indirect rôle, namely, by immobilizing one kind of ions without interfering with the mobility of other ions."[3] If methods of measuring the hydrogen ion concentration had been developed before colloidal behavior was investigated, "we should probably never have heard of the idea that the chemistry of colloids differs from the chemistry of crystalloids, at least so far as the proteins are concerned."[4] The measurement of the hydrogen ion concentration in investigating the chemical and physical behavior of proteins is of paramount importance, because "proteins are amphoteric electrolytes capable of forming ionizable salts with acids as well as with alkalies, according to the hydrogen ion concentration. . . . At the critical value of the hydrogen ion concentration the protein can practically combine neither with an acid nor a base nor a neutral salt. This critical hydrogen ion concentration is called the 'isoelectric' point of the protein."[5] This explains the fact that "solutions and suspensions of proteins are least stable at the isoelectric point."[6]

Colloidal phenomena proper are accounted for by Donnan's theory of membrane equilibria, which is based on the fact that "when a membrane separates two solutions of electrolytes one of which contains one ion which cannot diffuse through the membrane while all the other ions

[1] Moore, Benjamin, *The Origin and Nature of Life,* Chaps. V and VI.

[2] Loeb, J., *Proteins and the Theory of Colloidal Behavior.* Citations refer to the first edition of this book.

[3] Loeb, J., *op. cit.,* p. 2.

[4] *Ibid.,* p. 5.

[5] *Ibid.,* p. 5.

[6] *Ibid.,* p. 10,

can diffuse through the membrane, the result will be an unequal distribution of the diffusible ions on the opposite sides of the membrane. . . . This unequal concentration of the crystalloidal ions must give rise to potential differences and osmotic forces, and we intend to show that these forces furnish the explanation of colloidal behavior." [1] "Two laws of classical chemistry suffice to explain colloidal behavior quantitatively and mathematically, and these two laws are the stoichiometrical law and Donnan's theory of membrane equilibria." [2] Loeb presents a mass of qualitative and quantitative evidence in support of this thesis, and claims to have demonstrated its validity.

Into the merits of this and other controversies over the general subject we are of course not prepared to enter. We have only set forth some of the more important characteristics of colloidal behavior, together with what appears to be the leading interpretations thereof, in order to introduce such qualifications in the previous analysis as a liberal allowance for these phenomena might necessitate.

There is no doubt that colloidal behavior, whatever its ultimate interpretation, is a very important type of process in the living organism. Its precise significance is, however, far from clear, and, although there are striking similarities between the arrangement of some substances in the colloidal state, on the one hand, and the structure of "protoplasm" and of certain tissues, on the other hand, no real progress has been made toward an indentification of the two. Even if we shared fully the hopes now widely cherished of progress along these lines, the mechanistic hypothesis would scarcely be strengthened thereby. We should perhaps have reduced the number and even the kinds of the semi-independent bodies entering into the economy of the organism, and there organized; but we should have made no progress toward a proof that these bodies could organize themselves in the way required by the organism. The various types of physical change occurring in the body could not be reduced to terms of colloidal behavior, except to a very limited extent, nor could all the chemical reactions in the body be reduced to types of chemical action which *may be* exclusively associated with colloidal behavior; and all the numerically distinct physical and chemical processes falling outside the category of colloidal behavior, together with the numerically distinct actions of colloids themselves, would have to be organized according to the types of qualitative, quantitative, spatial and temporal specifications previously characterized.

The interpretation of such organization on the mechanistic hy-

[1] *Ibid.*, pp. 19–20.
[2] *Ibid.*, p. 26.

pothesis is therefore open to the same criticisms as those previously opposed to the hypothesis in its application to the organization of purely physical and chemical processes. We could of course pursue the topic into much greater detail—for example, by comparing the organization and activity of the cell with colloidal structure and behavior, or by considering chemical correlations in the body *between* as well as through colloids—but that would yield only a greater elaboration of the general result, not any significant qualification thereof.

Interpretations of Physiological Processes on Various Types of the Mechanistic Conception

We may dispense with any final summing up of the preceding analysis, and show how mechanists themselves have dealt with the problem of vital organization, when they have more or less adequately realized the existence and the difficulty of this problem.

Jacques Loeb, the leading representative of the elementalist school of mechanistic biologists, makes a show of recognizing and dealing with the problem, but when he gets down to business, which he usually does very promptly, he quite as promptly evades the whole problem, and takes for granted as much organization as may be needed for his mechanistic explanations. The artifice is so transparent that it is hardly worth while to dwell on it, for any one can see it in operation for himself by turning to almost any page of Loeb's writings where he purports to deal with "tropisms" or other alleged reactions of "the organism as a whole."

A favorite case [1] of his is the alleged rôle of heliotropism in preserving the life of the caterpillars of *Porthesia chrysorrhœa.* "This butterfly lays its eggs upon a shrub, on which the larvae hatch in the fall and on which they hibernate, as a rule, not far from the ground. As soon as the temperature reaches a certain height, they leave the nest; under natural conditions this happens in the spring when the first leaves have begun to form on the shrub. (The larvae can, however, be induced to leave the nest at any time in the winter, provided the temperature is raised sufficiently.) After leaving the nest, they crawl directly upward on the shrub where they find the leaves on which they feed. If the caterpillars should move down the shrub they would starve, but this they never do, always crawling upward to where they find their food. What gives the caterpillar this never-failing cer-

[1] *Der Heliotropismus der Tiere und seine Uebereinstimmung mit dem Heliotropismus der Pflanzen* (Wurzburg, 1889); summarized in *Forced Movements, Tropisms, and Animal Conduct,* pp. 161–162.

tainty which saves its life and for which the human being might envy the little larva? Is it a dim recollection of experiences of former generations, as Samuel Butler would have us believe?"

To this question Loeb returns a triumphant negative. "It can be shown," he says, "that this instinct is merely positive heliotropism and that the light reflected from the sky guides the animals upward. The caterpillars upon waking from their winter sleep are violently positively heliotropic, and it is this heliotropism which makes the animals move upward. At the top of the branch they come in contact with a growing bud and chemical and tactile influences set the mandibles of the young caterpillar into activity. If we put these caterpillars into closed test tubes which lie with their longitudinal axes at right angles to the window they will migrate to the window end where they will stay and starve, even if we put their favorite leaves into the test tube close behind them. These larvae are in this condition slaves of the light.

"The few young leaves on top of a twig are quickly eaten by the caterpillar. The light which saved its life by making it creep upward where it finds its food would cause it to starve could the animal not free itself from the bondage of positive heliotropism. After having eaten it is no longer a slave of light but can and does creep downward. It can be shown that a caterpillar after having been fed loses its positive heliotropism almost completely and permanently. . . . The restlessness which accompanies the condition of starvation makes the animal leave the top of the branches and creep downward—which is the only direction open to it—where it finds new young leaves on which it can feed. The wonderful hereditary instinct upon which the life of the animal depends is its positive heliotropism in the unfed condition and the loss of this heliotropism after having eaten. The chemical changes following the taking up of the food abolish the heliotropism just as CO_2 arouses positive heliotropism in certain *Daphnia*." [1]

This account of the behavior complex whereby the caterpillars of the given species are kept alive obviously consists of some careful observations, but it rests on numerous assumptions which are left unexamined. The hereditary organization of the caterpillar, its location on the shrub upon which the butterfly lays the eggs, its leaving the nest when the leaves on this shrub being to appear, the occurrence and disappearance of the positively heliotropic condition at the right stages of the caterpillar's development—all these elements in the caterpillar's behavior are taken for granted. If one were to penetrate deeper, the coördinations of manifold physical and chemical changes in the bodies, the activities and the environments of the caterpillar's ancestors, and the

[1] *Forced Movements, Tropisms,* etc., pp. 161–162.

coördinations of similar changes in the body, the activity and the environment of the given caterpillar would all have to be accounted for before explanation of the phenomena under consideration could be complete. The explanation offered by Loeb, even when all this is taken for granted, is largely based on unproved hypotheses. It is assumed that the light stimulus sets up *only* chemical or physical changes in the caterpillar, that its movements up and down the shrub are *purely* physicochemical phenomena, that the loss of the positive heliotropism on eating is an *exclusively* chemical process, and so on.

Is it not manifest that Loeb has evaded the larger part of the problem—even, from the standpoint of a comprehensive theory, the whole problem—and that he really does not grapple with those parts of the problem with which he does, after some fashion, deal? Absolutely the only thing proved by such investigations—and they are quite characteristic of Loeb and his school—is that, *given* the organism with all its structural and functional peculiarities, and *given* its position in the *given* environment, the *given* stimuli will induce the observed series of behavior. If more than that is done, it consists only in identifying specific chemical and physical changes in the body of the animal or its parts, and even these are often quite hypothetical. Of *organization,* which is the main problem, there is little or no account. It is taken for granted almost *in toto*.[1]

Of the same order is Loeb's interpretation of the effect of thyroid on the metamorphosis of the tadpoles of frogs and toads. This interpretation is based on experiments by Gudernatsch showing that,

[1] Jennings has demonstrated, on the basis of detailed observations, the impossibility of accounting for the behavior of lower organisms on the tropism theory. In summing up his discussion he says: "This theory uses and attempts to make of general application certain elements here and there observable in the behavior of some organisms. But in many organisms even these elements are almost completely lacking, and in no organism that we have taken up does this theory adequately express the nature of behavior. The tropism as applied to animal behavior in the sense we have considered, is not an elementary factor; it is only a more or less artificial construction, made by combining certain elements of behavior and omitting others that are of most essential significance. It makes use of certain simple phenomena that actually exist, but elevates these into a general explanation of directed behavior, for which they are utterly inadequate. The prevalence of this local action theory of tropisms as a general explanation of behavior in lower organisms is based only on an incomplete knowledge and an insufficient analysis of the facts of behavior." *Behavior of the Lower Organisms,* p. 274; Columbia University Press, publishers. See also, by the same author, "The Theory of Tropisms" in *Contributions to the Study of the Behavior of Lower Organisms,* Carnegie Institution Publication No. 16. For a criticism of the tropistic theory from the standpoint of the organismic school of physicochemicalists, see Ritter, W. E., *The Unity of the Organism,* Chap. XXI. For an appreciation and criticism of the theory, from the vitalistic standpoint, see Johnstone's *The Philosophy of Biology,* pp. 144 ff.

whereas normally tadpoles live from four months to a year before the legs grow out, these members can be made to appear at any time by feeding the tadpoles on thyroid gland. "We must, therefore, draw the conclusion," Loeb says, "that the normal outgrowth of legs in a tadpole is due to the presence in the body of substances similar to the thyroid in their action (it may possibly be thyroid substance) which are either formed in the body or taken up in the food." [1] Thus, thyroid or something similar to it is regarded by Loeb as a specific organ-forming substance for these animals. But Ritter has pointed out, [2] after reviewing Gudernatsch's experiments, that the "effect of thyroid food was to stop the increase in size of the frog's *larva* and start, almost at once, its transformation into the *adult*"; and that "thyroid substance is *organ-forming* only through being *organism-transforming*." We may add that in this as in the previous case, the structural and functional characteristics of the organism, in both its larval and its adult stages, are taken for granted. A more comprehensive and penetrating analysis would show here, as in that case, that innumerable chemical and physical changes are coördinated according to the most exacting qualitative, quantitative, spatial and temporal specifications, and that such coördinations are inexplicable in terms of the material processes that are coördinated.

Quite similar is Loeb's theory that differences between taxonomic groups are due to the differences in the protein compounds peculiar to these several groups.[3] Obviously any such theory raises in an aggravated form the difficulties of chemical correlation, the coördination of diverse types of physical and chemical action, the quantitative, spatial and temporal specifications to which physicochemical changes in the body must conform and, in short, all the difficulties which confront the mechanistic hypothesis in other physiological applications.

This criticism is scarcely weakened by the probability that proteins are in some way specific for different species and other taxonomic groups. For the facts of chemical correlation, the coördinations of chemical reactions according to more or less exact qualitative, quantitative, spatial and temporal specifications, are still there, and quite as unaccountable on any mechanistic conception of life.

It is unnecessary to adduce further cases. Clearly, if the criticisms earlier detailed have any validity at all, they apply to cases such as these, presenting as they all do and in a marked degree, the equilibration or organization of the specific physical and chemical processes oc-

[1] *The Organism as a Whole*, p. 156.
[2] *The Unity of the Organism*, Vol. II, pp. 143 ff.
[3] *The Organism as a Whole*, pp. 61 ff.

curring in the given organism, something inexplicable in terms of the discrete physical and chemical processes themselves.

Physicochemicalists of the organismic school have recognized the existence and, to some degree, the difficulty of the problem presented by the complex adaptive organization of physical and chemical changes in the body, and have attempted in various ways to deal with it, in conformity with the assumptions of their hypothesis. Biologists of this school have sharply criticized physicochemicalists of the elementalist school, and one of them at least has accused the elementalists of playing into the hands of the vitalists,[1] though conceding the value of the results secured by their methods. In working out their own theories, members of the organismic school attempt, of course, to steer clear of the fallacies charged against the elementalists, while still accounting for vital phenomena in physical and chemical terms. Perhaps the most thoroughly elaborated theory of this type is that developed. through several years of patient research, by C. M. Child;[2] and since we have considered in other connections types of organismic theory propounded by other workers, we may limit ourselves here to a brief examination of Child's theory.

Child shares fully in the criticisms urged by his school against the elementalists, but these need not be recapitulated anew, and we may proceed at once to a consideration of his positive conceptions.[3] It will be convenient to present these, for the most part, in Child's own words. "The orderly localization of parts," says Child, "and the orderly sequence in their appearance with reference to certain directions in the developing individual indicate the existence of some sort of ordering capacity underlying and preceding the stages where the order becomes structurally visible."[4] Hypotheses to the effect that correlations in the body are primarily chemical in character "provide no solution of the real problem of individuality, for they all involve the assumption of an underlying order or 'organization' which makes orderly chemical corre-

[1] Jennings, H. S., "Diverse Ideals and Divergent Conclusions in the Study of Behavior in Lower Organisms," *Am. Jour. Psych.*, Vol. XXI, 1910, pp. 349-370. W. E. Ritter's *The Unity of the Organism* is a plea for the organismic conception of life, and a systematic criticism of elementalist conceptions, whether or no explicitly based on mechanistic assumptions.

[2] Child's theory is really a combination of organismic and particularistic conceptions, however, as he assumes that the primary correlating agencies in the body are dynamic factors of some sort.

[3] For our purposes the analysis of Child's theory may be restricted to the more systematic exposition thereof in his three books, *Senescence and Rejuvenescence, Individuality in Organisms,* and *The Origin and Development of the Nervous System.* The University of Chicago Press is the publisher of these works.

[4] *Individuality in Organisms,* p. 7.

lation possible." [1] "The organism represents a pattern which involves the orderly arrangement and relation not of protoplasmic constituents but of protoplasmic masses, cells, and organs." [2] "The organismic pattern is fundamentally a molar, not a molecular or atomic pattern, for it involves regions consisting of many molecules usually of many different kinds." [3]

The correlating factors which produce the unity of the organism include chemical or transportative factors, however, and also mechanical or contactual, and dynamic or transmissive factors. But dynamic factors are primary in the processes of correlation, and the others are secondary to this group. The dynamic factors "consist in the transmission through protoplasm or along limiting surfaces of a dynamic change of some sort which is initiated in some particular region." The precise nature of these factors is not specified, though it is suggested that they may involve transportation of ions or electrons from one point to another; and they differ from the chemical or transportative factors in that correlation through their action is not accomplished by mass transportation from one point to another, but "by the passage of an energetic change which we call excitation or of some dynamic effect of excitation." [4]

Whatever the nature of this energetic change, it involves an increase in metabolic activity. A point of coördinate importance is "the existence of a decrement in intensity or energy of the change in the course of its transmission." [5] And the energetic change is positively correlated with metabolic activity, for it "originates in a region of high metabolic rate, and transmission occurs to regions of lower rate." [6]

Upon these facts and assumptions Child erects the doctrine of metabolic gradients, the distinctive concept of his theory. The organic individual consists of one or more gradients in cells, tissues or organs of specific physicochemical constitution. These gradients are of many different patterns, ranging from the comparatively simple surface-interior pattern of the cell, to the apical-basal and anterior-posterior patterns of complex organisms. Various complex relations of dominance and subordinance subsist between the gradients of higher organisms. These gradients determine "the direction of growth and differentiation and so are the basis of the geometrical space relations

[1] *Ibid*, p. 26.
[2] *The Origin and Development of the Nervous System*, p. 12.
[3] *Ibid.*, p. 5.
[4] *Ibid.*, pp. 12–15. See also *Individuality in Organisms*, p. 31.
[5] *Individuality*, etc., pp. 31–32.
[6] *Ibid.*, p. 29.

and the sequences in time which arise during the development of the individual. They may then be called axial gradients."[1] It is important to observe that the qualifying adjectives, metabolic and axial, do not denote the primary characteristics of gradients, but only secondary features thereof. Elsewhere, however, Child speaks of metabolism as the formative agent of the organism,[2] and of the organic individual as fundamentally neither a structural system nor a system of chemical reactions, "but rather a system of relations between a physical substratum or structure and chemical reactions."[3] This physical substratum is said to be colloidal in character, and the organism a colloid system.[4] The substratum itself, however, is a product of the dynamic changes conceived to be primary, but it influences the course and character of the latter.[5]

In general, the dominant conception is that of the axiate pattern defined as a series of "graded differences in the rate of the fundamental dynamic activities of protoplasm and in the conditions associated with these activities."[6] "Physiologically the most general feature of organismic pattern appears to be that of dominance and subordination, of controlling and being controlled."[7]

Metabolic or axial gradients originate in the interactions of protoplasm with its environment.[8] "The region of high metabolic rate results in the final analysis from the action of factors external to the mass acted upon, whether part of a cell or a cell mass."[9] More generally, "organismic pattern is not inherent in protoplasm, but arises in the final analysis from the relation of protoplasm to external factors;" and "the excitation-transmission relation is the most generalized and most primitive component in organismic pattern."[10] But, once an axial gradient is established in this way, it may "persist through the process of cell division or other forms of reproduction so that the unity and order of the new individual represent simply the unity and order of the parent or a part of it."[11] Both possibilities are realized in nature.

These various hypotheses, with exceptions to be noted later, are sup-

[1] *Ibid.*, pp. 35–36.
[2] *Ibid.*, p. 25.
[3] *Ibid.*, p. 29.
[4] *Senescence and Rejuvenescence*, p. 26.
[5] *Ibid.*, p. 26.
[6] *The Origin and Development of the Nervous System*, p. 24.
[7] *Ibid.*, p. 8
[8] *Individuality*, etc., p. 49.
[9] *Ibid.*, pp. 29–30.
[10] *The Origin and Development of the Nervous System*, pp. 17–18.
[11] *Individuality*, etc., p. 41.

ported by a variety of experimental evidence. And it must be granted that the existence of metabolic gradients has been demonstrated by Child and his coworkers, through experimental determination of differentials in susceptibility to the action of various external agents, in the penetrability of certain substances, the rate of certain oxidation-reduction reactions, oxygen consumption and CO_2 production, etc.[1] Evidence of various sorts is also adduced in support of the hypothesis that axiate patterns "arise through differential exposure to the action of external factors," this evidence including experimental modifications of axiate patterns.[2] The fundamental conception is admittedly left in a more hypothetical state, owing to the fact that the nature of the dynamic change, assumed by Child to be the primary correlating factor, has not yet been determined.[3]

This hypothesis undoubtedly has great heuristic value; and it represents a genuine contribution to the elucidation of the issues upon which it pronounces. The hypothesis has stimulated a great deal of valuable research and its influence along this line is by no means exhausted. Its contribution to the fundamental problems of vital organization, while genuine, seems to be very much more limited. The latter contribution might be characterized as in the nature of additional discoveries respecting the nature of that which is to be explained, but not in itself as supplying any part of that explanation. Otherwise stated, metabolic or axial gradients are a *descriptive* category of very great importance, but the phenomena subsumed under this category can hardly be accounted for in terms of the dynamic factors assumed for the purpose.

It needs only to apply our previous analysis in order to make this appraisal more explicit. The primary organizing factor postulated by Child is in the nature of energetic change, the precise character of which is not specified. Such change is assumed to be physical in character, whether of a sort or sorts known to us, or of some unknown sort or sorts that may later be discovered. In either case it would fall within the general category of physical action already established in the physical sciences. Any such factors would of course enter into various complex relationships with the several types of physical and chemical factors already identified in the organism. The resultant physical and chemical processes would all be in the nature of chance or accidental occurrences, considered with reference to the organization of these processes. We should then be confronted by the

[1] *The Origin and Development of the Nervous System,* pp. 28–49.
[2] *Ibid.,* Chap. III.
[3] *Individuality,* etc., p. 31.

identical difficulty before analyzed, that, namely, of accounting for the organization of these processes through their own chance or accidental occurrences. Child's axial gradients represent an enormously complex organization of specific energetical processes, whatever their nature may be, besides the chemical and mechanical correlations which are said to be secondary to the correlating factors constituted by those processes. There is the same type of organization according to fairly exact qualitative, quantitative, spatial and temporal specifications as before characterized, and the interpretation of this organization on a hypothesis of the type proposed by Child involves substantially the same consequences as does its interpretation on the elementalist form of the mechanistic hypothesis.

What Child evidently does is to assume that a particular type of physicochemical process constitutes the ordering capacity of the organism, and without developing the implications of that assumption. This failure to analyze his basic assumption is tantamount to taking for granted, in somewhat the same way as Loeb has done, the very thing to be accounted for, namely, the ordering capacity of the organism. The principal difference between them is that Child expressly recognizes the fact of such ordering capacity and the necessity of accounting for it, whereas Loeb does not.

It will not do to say, by way of rebuttal, that types of energetic change may be discovered which could constitute the ordering capacity of the organism, for all such types of change must conform in a general way to categories of physical or chemical action already established. As our previous analysis purports to show, nothing like vital organization can be in or derived from any known or conceivable types of physical or chemical action. The two categories are radically heterogeneous, and the reduction of one to the other is an impossible undertaking.

Mechanico-Teleological Conceptions of the Organism

We may take as a special type of the mechanistic conception Lotze's theory of the organism as a case of static teleology. We select Lotze as a representative of this theory because of the skill and learning with which he has expounded it, and because also of his historic importance as an opponent of the older vitalism. Since his criticisms of the latter doctrine do not here call for any rebuttal, we may leave them to one side and deal with his more positive conceptions.[1]

[1] The discussion will have reference to the final expression of Lotze's thought on the problem in hand. This of course is to be found in his *Microcosmus*. Citations will be from the English translation (Edinburgh, 1888), Vol. I. His attack

Organisms, on Lotze's theory, originate, like machines constructed by human hands, according to mechanical laws, and without any inner vital spring of action being concerned in the process.[1] Nature works just as does the chemist, the animal- or the plant-breeder, through the composition of natural forces. In all such activities the ultimate type of process is the movement of masses under the impulsion of purely mechanical forces. Living impulses, so-called, are only the summation of forces inherent in the constituent particles of the organism.[2] The order and regulative capacities of the organism are properties of a *system* of particles once for all established, and realized through the operations of the individual elements entering into this system. Once established, the operations of this system may be regarded as the movements of a machine, conforming to purely mechanical laws.[3] But the first arrangement of this system was purposive, and indicates "the traces of a wisdom that point us beyond the mechanical concatenation of mere events to an uncomprehended, creative Power." In reality, however, we know not whence this first arrangement of matter into living organisms originated.[4] Within the limits of our observation no new life arises, and the maintenance of life is dependent on the "uninterrupted transmission of certain substances with their particles in a certain conformation, as in propagation they are unceasingly transferred from one organism to another." [5]

In sharp contradiction with this mechanistic conception of the organism (as we think) is Lotze's theory of mind-body relationships.[6] Mental and bodily processes are, on his view, absolutely distinct and indeed incomparable; and the grounds of explanation for the two kinds of processes must be equally distinct and independent.[7] The traditional separation of supersensuous soul and material body is accepted,[8] while the theory that matter is endowed with a secret psychic life and the doctrine that mental states are epiphenomena of bodily processes are both rejected.[9] Lotze proposes, broadly speaking, an interactionist

on the older vitalism is presented in an article, "Life and Life-force," in Wagner's *Dictionary of Physiology* (Braunschweig, 1842), Vol. I. An analysis of the views expressed in this article will be found in Driesch's *The History and Theory of Vitalism,* English tr., pp. 127–132.

[1] *Microcosmus,* Vol. I, p. 18.

[2] *Ibid.,* pp. 19–23.

[3] *Ibid.,* p. 61.

[4] *Ibid.,* p. 65.

[5] *Ibid.,* p. 65.

[6] Cf. McDougall, W., *Body and Mind,* p. 82.

[7] *Microcosmus,* Vol. I, pp. 147–149, 261–262.

[8] *Ibid.,* p. 166.

[9] Elsewhere, however, Lotze attributes intelligence to the original supersensible elements which are said to have combined to form matter. *Op. cit.,* p. 433.

theory of mind and body. Bodily processes are all "signals for the soul to evolve definite internal states from its own essential nature"; [1] and they also serve to elaborate materials on which the soul exerts its energies. But bodily processes cannot constitute organs of understanding, reason or other mental functions.[2] In brief, bodily helps to the soul's activity are all "directed towards rendering possible, on the one hand, the combination of external impressions into a spatial arrangement of perception; on the other, the development of inner states to a purposive connection of spatial movements." [3]

The soul also has an exciting action on the body, but no attempt is made to specify in detail the nature of this action, other than to indicate some evidences of its occurrence.[4] In metaphysical terms, the soul acts, not on the body as such, but rather on supersensible beings combining in a definite way to give the phenomenal appearance of extended matter. Similarly, the action of the body on the soul is not that of matter or material instruments, but is in the nature of "psychical action and reaction, which alone contains life and energy." [5] "The image which we have now to form of the living form and its mental life is that of an association of many beings. The governing soul, placed at a favoured point of the organism, collects the numberless impressions conveyed to it by a host of comrades essentially similar but lower in rank from the inferior significance of their nature. Within itself it cherishes what it receives, fashioning it into motive impulses, which it applies to the ready force of its comrades, that thereby regular reactions may be evolved. A common understanding and sympathy pervades this combination, and nothing that happens to one part is of necessity lost to another." [6]

A point in Lotze's general conception of nature may be noted. Back of the mechanical course of events, both organic and inorganic, is the divine wisdom. Mechanism is therefore not the essence of things. "But nowhere does being assume another form of finite existence except through it; as we have not other gods beside God, so we need no other form beside this universal form of action in Nature." [7]

Lotze's theory of the animal organism, to which we shall largely restrict our critical analysis, is much the same as that espoused by the elementalist school of mechanists, since, on his theory, it is the forces

[1] *Ibid.*, p. 283.
[2] *Ibid.*, pp. 323–324.
[3] *Ibid.*, pp. 338–339.
[4] *Ibid.*, pp. 305–306.
[5] *Ibid.*, p. 364.
[6] *Ibid.*, p. 367.
[7] *Ibid.*, pp. 399–400.

inherent in the constitutent particles of the organism that produce "living impulses" and realize the order and regulative capacities of the organism. The principal difference between his theory and the ordinary type of mechanistic elementalism is his postulation of the original arrangement of matter, by some unknown power, into the organic form. This difference pertains to the origin and not the maintenance of living organisms. That being the case, our analysis applies without qualification to Lotze's theory, within the limits imposed upon the analysis. It need not therefore be repeated with reference to this particular form of the mechanistic hypothesis. The analysis, if valid, proves that regulative capacities, or organizatory factors, are at work throughout the entire range of vital activity, and that these factors are not reducible to terms of physicochemical processes. Consequently, the analysis refutes Lotze's conception of the maintenance and propagation of life, with its interpretation of the order and regulative capacity characteristic of life.[1]

We may say, in addition, that the existence of organizatory factors in vital activity, assuming this to have been demonstrated, renders unnecessary Lotze's postulate of an original arrangement of matter, by some unknown power, into the organic form. For we seem justified in assuming that life was originally organized by factors of the same general category as are now operative in vital activity.

Into Lotze's theory of body-mind relationships we shall not go in any detail, as this subject has been considered in previous chapters, and we have presented Lotze's doctrine on this problem only because it is an interesting attempt to combine an interactionist conception of the mind with a thoroughgoing mechanistic conception of the organism. We need here only refer to the analysis in a previous chapter, showing that the actions of animal, particularly human, organisms directed toward distant objects with which they are not effectively related in a physical sense, must be determined in part by nonphysical, or psychical, factors. If this analysis be correct, it supports Lotze's interactionist conception of mind, but refutes his mechanistic conception of the organism. For action guided by mental processes is more than mechanical action, though involving the latter.

[1] We may recall attention to the intent of the analysis, whose critical implications for Lotze's theory are here indicated. That analysis purports to show *why* physical and chemical processes cannot in themselves determine the order, organization or regulative capacities of the organism, whether or no this organization be conceived on the analogy of a machine. That the organism *is not* a machine has now been conclusively demonstrated on the basis of empirical evidence. See, for the more important of this evidence, Haldane, J. S., *Mechanism, Life and Personality*, pp. 53 ff.; Driesch, H., *The Science and Philosophy of the Organism*, Vol. I, pp. 138–141; Wagner, A., *Der Neue Kurs in der Biologie*, pp. 44–50.

No fresh treatment of the problems here under consideration can be even relatively complete, that does not allow for Henderson's discussion thereof in two noteworthy books recently published.[1]

Henderson contends that environmental fitness is as real as organic fitness, and quite as significant as the latter, considered from the purely biological point of view. He presents an analysis of the environment, demonstrating its fitness not only for the living organism but for "systems" generally, including the atmosphere, the soil, the ocean, etc. He shows, in particular, that life is peculiarly dependent on the unique properties of three chemical elements and two of their compounds. These are hydrogen, oxygen, carbon, water and carbonic acid. That this ensemble of unique properties, and particularly their complex connections with the characteristics of systems, should occur by accident is, according to Henderson, almost infinitesimally small. This ensemble of properties therefore possesses a teleological character.

Organisms are also teleological in character. But the teleology in no system is incompatible with the detailed operation therein of purely mechanical laws. The teleology operates alongside the physical elements, as their predetermined and necessary associate. Vitalistic hypotheses are rejected in favor of this universal teleology, and, more particularly, because there are no more grounds for assuming the operation of extra-physical influences in the organic than in the inorganic, both being characterized by fitness and exhibiting purposive tendencies in about the same degree. But teleology is sharply contrasted with mechanism, and their conjoint operation presents a riddle which we cannot solve, for it concerns the origin of things. This contrast, according to Henderson, is the very foundation of the order of nature. It should be added that Henderson fully recognizes the defects of current mechanistic conceptions, but he holds that scientific investigations must be guided entirely by such conceptions.

Henderson appears to have made an important contribution to theoretical biology, in emphasizing the physicochemical factors in vital activity, and in criticising vitalistic conceptions that tend to underestimate the significance of these factors. Moreover, he has revived, in a critical form, the question whether the determination of the properties of inorganic substances, and particularly the distribution of such substances on the earth's surface, were not in a genuine sense a preparation for life and its evolution. More generally, he has emphasized, as against uncritical mechanistic conceptions, the claims on our attention of the teleological aspects of nature, and shown that these present problems not to be evaded, even though they should prove

[1] Henderson, L. J., *The Fitness of the Environment,* and *The Order of Nature.*

insoluble. All these are noteworthy contributions, whatever the final verdict on Henderson's positive conceptions may be.

The value of his contributions to the analysis of vital organization, however, seems much more limited. The teleology of the organism is radically unlike other kinds of teleology, if there be such; and must be sharply distinguished from those other teleologies, if its precise significance for vital activity is to be understood. Much of our previous discussion has been directed to this point, and the results of this discussion must be left to speak for themselves, as against conceptions of the sort advocated by Henderson. But some of our conclusions may be compared with Henderson's views on the same questions.

In the first place, the types of relationships that characterize physiological activity are not explainable, as we contend, in terms of physicochemical processes. Henderson holds the contrary, assuming, as he does, that all systems are mechanistic in character, and that their teleological character is not incompatible with the universal operation of physical and chemical laws. In our judgment, the organism on its physiological side is differentiated so radically from inorganic systems that the assumption of a category of factors not found in the inorganic is necessary to account for this difference.

In the second place, as will be shown more fully hereafter, these factors appear to be characterized by something analogous to human intelligence (it may be a type of intelligence), for otherwise the successful adaptation, in development, of means to the ends to be attained, and the regulative processes of the organism in relation to the exigencies that often confront it, would scarcely be intelligible. The same proposition is supported by our analysis of the genesis of hereditary active adaptations, particularly adaptations of which functional activity could not have been an efficient cause. This attribute of the organism differentiates it still further from all inorganic systems.

Finally, the guidance of animal behavior with reference to distant objects, by non-physical factors, shows still more clearly that the reign of mechanism is far from universal, and that the teleological factors of the organism are sharply contrasted with teleological factors elsewhere, if there be such.

Henderson creates rather gratuitous difficulties for himself, it would appear, by his unwillingness to assume the existence of factors in nature that are not physical in character, and the peculiar mystery which the teleological appearance of nature takes on, as a consequence. This mystery ceases to be one in any special sense, so far as the organic is concerned, when we assume that the teleological character

of vital activity is due to quasi-intelligent organizatory factors; and also, in the case of animal organisms, to non-physical factors endowed with intelligence, in the more common sense of the term.

The Relational Theory of the Organism

There is also a *relational* theory of the organism, which merits consideration in connection with the present problem. Spaulding is an advocate of this theory,[1] and we shall comment briefly on his version of it.

According to Spaulding's *general* theory of nature, both organic and inorganic wholes are formed by a creative, non-additive organization of their parts. Such organization is effected by what he terms "organizing relations," and these are specific to each distinct sort of whole that is formed. All these wholes have properties that their parts do not have, and are therefore not reducible to terms of their parts (or elements), though they imply the latter.

Organizing relations are as characteristic of inorganic as of inorganic wholes, and this, insofar, obviates the necessity of assuming the presence in the organic of entelechies or other special organizing agents. Moreover, later stages in the behavior or development of the organism can be sufficiently explained in terms of the earlier stages, just as can later stages in a series of inorganic events. Also, organisms do the same things under the same circumstances, just as inorganic wholes do; and the same effects are often produced in the inorganic, as in the organic, by different causes. This line of evidence, according to Spaulding, shows that vitalistic hypotheses are unnecessary to account for the organism and its activity.[2]

Our comments on this theory will be very brief. Spaulding's contention that organic wholes do not differ so radically from inorganic wholes as to require special principles to account for them we cannot accept. What we said as to this feature of Henderson's conceptions applies to Spaulding's position, and need not be specially qualified in relation to the latter.

But Spaulding asserts that the organizing relations which form the organism are specific, that is, different from organizing relations which form inorganic wholes. Perhaps, therefore, criticisms of his doctrine should be directed mainly to his relational conception of nature in

[1] *The New Rationalism, passim,* esp. pp. 448–449; "A Defense of Analysis," in *The New Realism,* esp. pp. 243–247; "The Energy of Segmentation," *Jour. of Exp. Zool.,* Vol. IV, 1907, pp. 283–316.

[2] *The New Realism,* pp. 244–247.

general, rather than to the application of this conception to any particular domain of nature. Let us, therefore, briefly examine this conception.

Spaulding appears to hold that the various organizing relations are something over and above the elements synthetized by them into new wholes,[1] and yet to maintain that the organism (for example) is a specific *physicochemical* complex.[2] This seems to be a contradiction, unless it is assumed that the specificity of the organism is at least partially due to the specific organizing relations involved therein, and not alone to its physicochemical conditions; in which case, it would hardly appear appropriate to term the organism a physicochemical complex, unless warning be given that the *complex* embraces more than physicochemical processes.

Now, if Spaulding's theory *is* a mechanistic one, in the physicochemical sense of the term, it is of the organismic type, and open to the same general criticisms as other instances of this type. If he holds, on the contrary, that organizing relations in the organic are not physicochemical in character, a different criticism would apply. To the questions involved in this alternative construction of his theory we cannot attempt here to do justice. We would only venture the comment that the conception of relations without terms, which this construction implies, seems to us at least a logical monstrosity, though it may be admitted that terms must be largely defined by means of their relations. To our way of thinking, organizing relations presuppose organizing factors or agents; and there would appear to be no more grounds for refusing to assume the existence of such factors, than of "intensity-points" from which, as Spaulding holds, the electron and other physicochemical wholes are built up.[3]

Summarizing our discussion of physicochemical conceptions of the organism on its physiological side, we seem justified in asserting that none of these conceptions has succeeded or, on our showing, could succeed in putting into physical and chemical processes the types of relations which distinguish physiological activities from inorganic events; and for the reason that relations of these types are not in or derivable from physical or chemical processes. The qualitative, quantitative, spatial and temporal relationships which characterize the physiology of the organism are radically heterogeneous with the relations studied by physics and chemistry; and must be superadded to the physical and

[1] *The New Rationalism,* pp. 446–448.
[2] *The New Realism,* pp. 244 ff.
[3] *The New Realism,* pp. 237 ff.

chemical processes of the body before the essentially contingent character of the latter can enter or be transmuted into the adaptive organizaion which distinguishes organic from inorganic systems.

We reserve for a separate chapter the examination of mechanistic theories of hereditary variations, one of the principal problems of our inquiry, and that which served as our original point of departure. Following that, we shall test, by the method of concomitant variations, physicochemical conceptions of the organism and its properties. This will serve as an independent check on results exhibited in the discussion prior to that, as yielded by the application of different tests to the same group of conceptions.

CHAPTER X

MECHANISTIC THEORIES OF HEREDITARY VARIATIONS

Attention will be centered in this chapter on the chemical and mutationist theories of hereditary variations. These of course are not mutually exclusive, but commonly combined together; yet they should be distinguished for purposes of analysis. Mendelian principles of heredity and the chromosomal hypothesis respecting the germinal basis of Mendelian characters will be considered in their relation to the mutationist theory of variations. The Weismannian theory of variations may be and usually is regarded as a mechanistic conception, but it will receive only incidental consideration here, as it has been examined at length in our discussion of anti-Lamarckian hypotheses. Some incidental consideration will be in order for the reason that the hypotheses on which the discussion is to be centered are usually formulated in such a way as to carry anti-Lamarckian implications, a fact which is traceable to Weismann's influence. Other anti-Lamarckian hypotheses and the Lamarckian hypothesis itself are ordinarily given a mechanistic slant, but it will not be necessary to bring them into the present discussion other than by barely indicating the bearing of the discussion on mechanistic constructions thereof. Some provisional treatment was previously given the chemical interpretation of the Weismannian (or germinal) hypothesis, and the examination of the chemical theory in the present chapter may be taken as completing and generalizing the results of that treatment.

The Weismannian, the mutationist and the explicitly chemical theories of hereditary variations may be regarded as representative types of mechanistic theory on this problem, the chemical theory standing, with necessary qualifications, for physicochemical conceptions of variation generally, the Weismannian and mutationist theories for mechanistic conceptions not explicitly defined in physicochemical terms. Gaps in our survey of mechanistic hypotheses respecting the origin of variations can be readily filled in by the requisite extensions of the analysis to which the representative types of those hypotheses is subjected. We shall first examine the chemical hypothesis respecting the origin of hereditary variations, and then briefly indicate qualifications of the

analysis which will render it applicable to physicochemical hypotheses generally.

The Chemical Theory of Variation and Heredity

Chemical alterations of the germ-plasm can be nothing more than the production, through physicochemical action therein, of molecules of a different formula, or formulae, from those previously organized in the germ-plasm, or a change in the number of molecules of one or more formulae, or both types of alteration—the qualitative and the quantitative—put together; and the actions of these new kinds of, or of these added or subtracted, molecules must be in the nature of chance or independent occurrences, considered with reference to molecules already organized in the germ-plasm, and also with reference to the somatic part or parts alleged to be modified exclusively by the given chemical alterations. That is, the sum total of chemical actions starting from purely chemical changes in the germ-plasm could not be organizatory in nature, and could not by themselves alone, therefore, determine *reorganization* either of the germ or of the soma. The difficulties of assuming that coördinated modifications of the soma can be determined by chemical changes in the germ-plasm are greater than those of assuming that the germ-plasm can be so modified, for in the former case an *organized series* of organized changes occurs, which involves an enormous number of chemical bodies, each bearing specific qualitative, quantitative, spatial and temporal relationships to all the others. Chemical (and physical) processes alone could hardly determine so complex and specific an organization of these same processes. Besides, this hypothesis takes for granted the preëxisting organization of both the germ-plasm and the soma and this would have to be accounted for by the hypothesis, since this preëxisting organization must, according to the hypothesis, have been built up by chemical agencies similar to those alleged to determine any given hereditary changes in this organization.

These difficulties are in no wise obviated by a chemical theory of heredity and of hereditary variations such as Guyer proposes.[1] Guyer holds that fundamental substances specific to the given species and probably protein in nature for the most part, together with equally specific enzymic substances which regulate the chemical and physical processes of development, constitute the mechanism of heredity. The

[1] "Nucleus and Cytoplasm in Heredity," *Am. Nat.*, Vol. XLV, 1911, pp. 284–305. See *supra*, pp. 171–172 for a statement and criticism of Guyer's suggestion that serological reactions may constitute a mechanism whereby somatogenic characters are impressed on the germ-plasm.

complexity of the chemical and physical processes in the cell and the necessity of postulating "some time-, quantity- and quality-controlling mechanism" are fully recognized. The enzyms constitute this mechanism. The chromosomes are regarded as "a sort of gauge for the feeding out of enzyms at the proper rate to bring about proper velocity reactions in the other cellular constituents." With the development of the germ other chemical and also non-chemical influences of one part on another come into play, the hormones, for example, being an important factor in the development of complex organisms. Hereditary variations, including "ontogenetic short-cuts" and racial reversions, are interpreted on the basis of these conceptions.[1]

The application of our analysis to this hypothesis is so obvious that it need not be indicated in any detail. The hypothesis assumes in effect that the discrete chemical and physical processes of the cell, admitted to be very diverse and numerous, can regulate themselves according to the qualitative, quantitative and temporal specifications expressly recognized by Guyer to be binding. The "controlling mechanism" is described first in terms of enzyms and then in terms of chromosomes, but we are not told what it is that determines the production of the right sorts and amounts of the enzyms, not to mention the proteins and other specific substances of the cell; nor are we told what it is that arranges the chromosomes in such a way that they can feed out the enzyms at the proper rate. We clearly have here organization of a very complex character, involving the types of specific relationships between the material processes organized, which have been mentioned. These relationships, we submit, are not in or derived from those processes, but are in some way superadded to them.

Let us now examine the application of this hypothesis to hereditary variations themselves, dealing first with variations that result in new intra-organic adaptations, or the modification of existing ones. Apparently the great majority of hereditary variations involve adaptations of this class, since all tissues of the body are adapted to or coördinated with other tissues, and any substantial change in the shape, relative size or functions of a given tissue must necessarily involve changes in correlated tissues.

Even admitting that change in the character or in the number of molecules in the germ-plasm could determine hereditary alterations in a particular somatic tissue, changes in coördinated tissues would have to be introduced, too, if the change in that tissue was of such a nature as to require it. Both series of changes would have to be explained as the result either of a single change in the germ-plasm, or of com-

[1] *Op. cit.*, pp. 303–304.

plementary changes therein. In either case, we have changes of an organizatory nature, and such changes cannot be ascribed, according to our analysis, to chemical actions alone, since these are in the nature of chance happenings, considered with reference to any organization of those actions, and so could not result in organization of this nature.

The same analysis applies to the interpretation, on this hypothesis, of active hereditary adaptations to the environment. A variation representing the genesis or modification of a hereditary adaptation to the environment must usually involve a large number of tissues, since the behavior of the entire organism is modified in one or more particulars. And there are the same difficulties in a chemical interpretation of these complementary or coördinated histonal changes having reference to features of the external environment, as in the corresponding interpretation of coördinated histonal changes in the body having no explicit reference to the external environment.

The difficulties of the chemical hypothesis are, in fact, accentuated when applied in the interpretation of hereditary active adaptations to the external environment. For the organization in these cases embraces not only the coördinations of the germ-plasm with the soma, but also the coördinations of both these with the features of the external environment to which the given adaptations have reference; and this complex organization involves an all but infinite number of processes that are coördinated together according to numerous but very definite *non-chemical* specifications—qualitative, quantitative, spatial and temporal. Moreover, many hereditary active adaptations to the environment, particularly the instincts, involve *psychical* factors, a category of factors we have distinguished from the organizatory factors involved in the purely physiological activities of the organism. And such adaptations bring into play at least one type of relations not previously specified as characterizing the functional activity of the organism, namely, *logical* relations. It is possible that relations of the same type are involved in the organizatory processes of the body generally, since, as we shall see later, all these processes appear to be under the control of something akin to human intelligence. In any case, the implication of logical relations in at least some phases of hereditary vital organization multiplies the difficulties of the chemical hypothesis, since such relations could hardly be in, or derived from, the chemical processes involved. Finally, the difficulties of this hypothesis are accentuated still further when its application to hereditary *changes* in complex vital organization of this type is considered.

Our general criticism of the chemical theory applies also to its interpretation of hereditary variations not involving active adaptations, at

least insofar as such variations depend on the organization of chemical processes according to non-chemical (or non-physical) specifications of the sorts before indicated. This is an obvious deduction from the analysis presented in the previous chapter. Apparently most if not all such variations do involve organization of this type.

When to the chemical theory of hereditary variations is annexed some one of the various anti-Lamarckian interpretations of such variations, the difficulties of the particular anti-Lamarckian interpretation, as detailed earlier, are added to it. When combined with some form of the Lamarckian hypothesis which is otherwise tenable, these particular difficulties are not, of course, encountered. As we shall see later, however, the Lamarckian hypothesis itself is hardly compatible with a consistently chemical interpretation of hereditary variations.

The qualifications in the analysis of the chemical theory of hereditary variations required to render it applicable to physicochemical theories of variations taking into account colloidal behavior and purely physical processes in the organism are readily deducible from our previous treatment of physicochemical conceptions of life which also take these phenomena into consideration. We may say, without setting forth such a deduction in detail, that the organization exhibited in the genesis of hereditary variations cannot, according to that analysis, be reduced to terms of the physical and chemical processes involved therein; and that the greater number and complexity of the physicochemical processes allowed for by this type of the hypothesis emphasizes the difficulties of accounting for their organization in terms of themselves alone. The anti-Lamarckian hypotheses interpreted on this broader physicochemical basis would of course all involve their peculiar difficulties, as earlier detailed, and the Lamarckian hypothesis so interpreted would involve the difficulties peculiar to this form of the physicochemical hypothesis.

Mutationist Conceptions of Hereditary Variations

We shall now discuss hereditary mutations and Mendelian inheritance, insofar as these topics come within the scope of our analysis. We may here disregard the physicochemical interpretation of these phenomena which is often annexed to them, and regard them simply as evidence alleged to support a mechanistic conception of life not explicitly interpreted in physicochemical terms. We shall accept at its face value the hypothesis that the chromosomes and their processes constitute the germinal basis of Mendelian characters and their behavior in inheritance; and not consider the question whether the hy-

pothesis accounts for all the facts in the case, or the further question whether it will ever be possible to reduce all the hereditary characters of the organism to terms of Mendelian characters, an assumption commonly accepted by present-day geneticists.

Mutations are now assumed by most geneticists to be due to the emergence in the chromosomes of one or more new factors, or to the modification of existing factors, or to the loss of existing factors. There is now a tendency to ascribe mutations to the emergence of one or more new factors, or to the loss of old ones, though the significance of this tendency seems to be of a terminological order. It is now generally agreed that mutations may be large or small, that they may affect one or several parts, and that they may be profound or superficial in character. The question whether the larger, profounder and more extensive mutations play a large or a small rôle in evolution is not yet settled. There is in any case an enormous number of particulate factors in the hereditary material of the organism. It has been shown that each mutant gene may have, and probably always has, manifold effects on the hereditary characters of the organism, while, on the other hand, "many genes, and perhaps a very large number, are involved in the production of each organ of the body. It might perhaps not be a very great exaggeration to say that every gene in the germ-plasm affects several or many parts of the body; in other words, that the whole germ-plasm is instrumental in producing each and every part of the body. . . . *Yet this germ-plasm is made up of units that are independent of each other in at least two respects, viz., in that each one may change (mutate) without the others changing, and in segregation and in crossing over each pair is separable from the others.*" [1]

The questions raised by these facts and these hypotheses which specially concern us are (1) whether this apparently independent variability of each gene can be reconciled with our theory of correlated variation in interadapted parts; and (2) whether a single mutation in a gene can correspond to modifications of two or more somatic characters which are coördinated with or adapted to one another.

In answer to the first question, it may be pointed out that the genes must each have its place in a germinal *organization* of some sort, since all of them taken together (assuming that all the hereditary material is composed of genes) develop into a single organism, and by a continuous, complex series of changes. If they do not themselves form a genuine organization, they have explicit reference to such an organization, the developed soma. Moreover, the behavior of the

[1] Morgan, T. H., *The Physical Basis of Heredity*, pp. 240–241.

cell in division, maturation, fertilization, etc., and particularly the behavior of the chromosomes, is direct evidence that the genes are organized. It is quite certain that all somatic characters which may mutate are organized, or coördinated with other characters, and in the last analysis with all other characters, since all taken together form a single organism. Now, hereditary mutation cannot normally be of such a character as to throw the organism entirely out of gear. The very notion is, of course, self-contradictory. Whenever a mutation in one part of the body is of such a nature as to require mutations in other parts, those parts must mutate in the requisite manner or the organism will perish. There is no other alternative. If it is a mutation in the relative size or shape of a bodily structure, other structures must be changed to fit, or new structures produced, for otherwise the organism will not hang together. If the mutation is one in function, as in a digestive process, for example, there must be corresponding changes in other functions, or the organism will perish—provided, of course, the given functional change is of such a nature as to necessitate correlative functional changes. There is no escape from the conclusion that the genes (or other ultimate factors in the germ-plasm) are correspondingly organized, and that therefore the independent variability of each gene is a very limited and comparatively insignificant phenomenon, if it occurs at all.

Our second question is whether mutation in a single gene could produce modifications in two or more somatic characters which are correlated with or adapted to one another. This question is virtually disposed of by evidence showing that many genes and perhaps the entire germ-plasm are instrumental in producing every part of the body, evidence which is compatible with the assumption that the genes coöperate in the production of hereditary changes in the soma, and incompatible with any contradictory assumption. It is impossible to maintain, therefore, that a mutation of a *single* gene could determine correlated changes in two or more somatic characters, or even a change in a single character which did not at the same time necessitate a correlative change in some other character. The evidence tends to show that neither of these things happens. But assuming that the mutation of a single gene *contributes* to the causation of correlative changes in two or more somatic characters, then this gene and its mutation would represent two or more such characters and changes therein, whereupon it follows that the given gene is not a simple but a complex factor, or complex of simple factors. We may say, therefore, that correlated hereditary variations in two or more somatic characters necessarily involves correlated variations (or mutations) in a correspond-

ing number of genes or germinal factors. (The evidence indicates of course that a larger number of germinal factors is involved.) This means, again, that the genes or other ultimate units of the germplasm must be organized and that their mutations must also be organized.

We may say, finally, that the organization of the genes and of their mutations is not wholly reducible to terms of themselves and their behavior, when the genes are regarded as independent entities or variables. Such a conception would of course be self-contradictory. But should it still be entertained as a serious hypothesis, then it becomes subject to criticisms paralleling those directed against the chemical theory of hereditary variations.

If the mutationist and factorial hypotheses as here considered be explicitly linked with the physicochemical or with one or more of the anti-Lamarckian hypotheses, the difficulties of such hypothesis or hypotheses will be added to them. All this means that the phenomena of mutations and of Mendelian inheritance must be interpreted, where a comprehensive analysis is in question, on some such synthetic theory as we have proposed, which allows for the operation both of the Lamarckian principle and of organizatory factors in the genesis of variations, while not discounting the part played therein by physical and chemical processes.

CHAPTER XI

INCOMPLETE CORRELATIONS BETWEEN VITAL AND THE ASSOCIATED PHYSICOCHEMICAL PROCESSES

THE discussion in the previous chapters showed that mechanistic conceptions of life are incapable of accounting for the psychical attributes of the organism, or for the organization of physicochemical processes represented by its physiological activities. In the present chapter we propose to test mechanistic interpretations of the same groups of characteristics, by the method of concomitant variations. We shall first analyze, by this method, physicochemical interpretations of various morphological characteristics of the organism.

CORRELATIONS BETWEEN PHYSIOLOGICAL AND THE CONCOMITANT PHYSICOCHEMICAL PHENOMENA

One very significant fact is the persistence of organic structure with but little change generation after generation, and in some cases for entire geological epochs, while the particular chemicals and energies associated with that structure are constantly changing. This fact obviously signifies an incomplete correspondence between the variations of organic structure and the configurations of matter and energy combined with it. The structure is highly stable, while the individual configurations of matter and energy associated therewith persist for only brief periods of time. Individuals die and the individual configurations of matter and energy undergo dissolution, while the structure, the species, persists.

Moreover, structures evolve, giving rise to differentiated races or species, while the chemical compounds and forms of energy associated with structure do not evolve, or at least do not evolve at the same rate or in the same direction. "The underlying physicochemical processes in living organisms," says Henderson, "seem to have remained about the same throughout the whole process of evolution. So far as it is possible to form any opinion on the matter, this conclusion is inevitable." [1]

[1] Henderson says further, in the paper quoted:

"With very trivial exceptions, the economy of life on the earth is now and probably always has been founded upon the synthesis of carbohydrates from water and car-

Nor is there in ontogeny any strict correlation between changes in the typical physicochemical processes of the organism, and concomitant changes in its morphological and functional characteristics. While there is some degree of positive correlation between series of changes falling within the two categories, as in the development of secondary sexual characteristics, such correlation is not very high relative to the scope and nature of those changes.

Moreover, organisms of the same species all have their individual characteristics, comparable in a general way with the characteristics of the species itself; but conclusive evidence is wanting that the physicochemical processes of individual organisms differ so extensively as do their morphological and functional characteristics.[1] While each individual may differ from all other species or individuals physicochemically—and there is some evidence that this is the case [2]—, these differences pertain not so much to the specific chemical compounds synthetized in the body, as to the individual *systems* of physicochemical interactions, and probably also to the proportions of the resultant end products to one another. Certainly there is no correlation between the number of distinct bio-chemical compounds and the

bonic acid with the accompanying fixation of energy, followed by the conversion of carbohydrates into fats, proteins, and a great variety of other related substances. Later there is an oxidation of these substances back to water and carbon dioxide, accompanied by the utilization of the energy in various forms of organic activity. Correlated with this is the fact that cells are made up of water, carbonic acid, carbohydrates, fats, proteins, and certain other substances. They are enough alike in chemical composition and in physicochemical structure fully to justify the concept of protoplasm as a fairly constant physicochemical apparatus throughout the organic world. . . .

"Thus while the evolutionary process has certainly produced a large number of well-defined series of changes when it is looked at from the morphological point of view, it still remains very probable that such physicochemical changes as have occurred are not only of a secondary nature, but that they are much less of the character of serial modifications. Indeed, one is tempted to say that in a physicochemical sense, the variations are distributed in rather a random manner, without any particular indication of a general progressive tendency, such as we seem obliged to think of in studying morphological variation.

"No doubt the evolutionary process has, from time to time, invented new chemical substances and greatly modified colloidal systems. In the total these changes are very numerous and of the utmost importance to the student of evolution. But *progressive* change is more particularly a morphological phenomenon and it seems to be almost self-evident that progressive morphological evolution should not be accompanied by the same degree of continuous variation in straight lines in physicochemical properties. Such a parallelism would, I think, be well nigh unaccountable. However that may be, there is no evidence for it." Henderson, L. J., "Orthogenesis from the Standpoint of the Biochemist," *Am. Nat.*, Vol. LVI, 1922, pp. 99, 101.

[1] The same observation would apply to different organs and tissues of the same organism, and to corresponding organs and tissues of different organisms.

[2] Summarized by Ritter, W. E., *The Unity of the Organism,* Vol. I, Chap. IV.

number of species or of individual organisms which have appeared in the world.

Two different types of imperfect correlations between bodily structures and the associated physicochemical processes are indicated by these facts. (1) There is an all but complete lack of correlation between those structures—whether of species or of individual organisms—and the *typical* physicochemical processes associated therewith. (2) There is also an incomplete correlation between those structures (with the corresponding functions) and the *particular* chemical compounds and energies associated therewith. Stated otherwise, organic structures and functions are, to a marked degree, independent of (1) the *types* of physicochemical processes and (2) the *numerically identical* physicochemical prosesses associated with structure and function. The type of structure and function is obviously not dependent on the type of physicochemical processes associated with it; and the individuality of structure and function is not dependent on the individualities of the chemicals and energies associated with it. So far as the criterion of concomitant variations supplies a test of physicochemical hypotheses, therefore, these hypotheses are proven to be untenable. Such measure of correlation between the two series as has been demonstrated does not seriously affect this conclusion, as these correlations cover at most but a small and relatively insignificant portion of the facts to be accounted for.

The results so far attained by the analysis are readily comparable with the results yielded by the discussion in previous chapters. The morphological and functional characteristics of the organism considered in the present chapter embrace the quantitative, qualitative, spatial and temporal relationships among bio-chemical and bio-physical processes, previously considered, and found to be not completely reducible to terms of those processes; and they also take in organized changes in these relationships discussed in the previous chapter under the heads of variation and inheritance. And the differences between the morphological and functional characteristics of individual organisms, as discussed in the present chapter, are clearly correlated with the types of relationships among the physicochemical processes of the organism, before considered.

Correlations Between Mental and the Concomitant Physicochemical Processes

We shall now compare by the method of concomitant variations certain psychic functions of the human organism and the associated

processes of the brain and nervous system, considering first the correlations between individual instances of such functions and the numerically identical chemicals and energies associated therewith.

As everybody knows, chemicals and energies are constantly exchanged between the organism and its environment; and it is fairly certain that numerically identical chemicals and energies do not remain permanently in tissues which undergo waste and repair, as do nervous and all other living tissues. A series of experiments by Shiro Tashiro [1] has shown that oxidative metabolism in the brain and nervous system is more intense than in other tissues of the body and, therefore, that certain substances therein are broken down and displaced with greater frequency than are corresponding substances in other tissues. There is some evidence, however, that the protein compounds in the brain and nervous system are broken down and displaced more slowly than are the proteins of other tissues. It has been estimated on the basis of experimental data that if the decomposition of proteins in the tissues were equally distributed, such proteins would be renewed once in five years.[2] There is no basis for an exact estimate of the rate by which protein decomposition in the tissues of the brain and nervous system occurs, but we can assume a much lower rate of protein decomposition therein, and still have a basis left for some significant comparisons of the probable variations in the physicochemical processes of the brain and nervous system, with changes in the associated psychic functions.[3]

Now, as we all know, memories often persist from childhood through adult life, and there is evidence that habits and other acquired mental functions also persist for considerble periods of time—longer, in all probability, than the time requisite to a complete displacement of the chemicals and energies present in the brain and nervous system at the time such functions were acquired. Bourdon, a French psychologist, acquired various habits and tested their retention after short intervals of time, and again after seven or eight years had elapsed, without practice of those habits in the interim. It was found that such functions as the translation of a certain number of German words into French, or naming a certain number of parts of speech,

[1] *A Chemical Sign of Life,* pp. 29 ff., 36.

[2] Mathews, A. P., *Physiological Chemistry,* 3d Ed., p. 804.

[3] The analysis here has special reference to the proteins of the brain and nervous system because these are presumably the most stable compounds therein. Mathews has suggested that memory hormones are produced and retained in the nerve cells (*op. cit.,* p. 590). While this suggestion is an interesting one, there is no experimental evidence which supports it, and it is impossible to conceive the synthesis of such hormones from the enormously complex physicochemical processes associated with the acquisition of particular memories.

were retained for seven or eight years without practice, and with comparatively little loss of efficiency.[1] Moreover, there is no doubt that certain types of habits, such as those involved in swimming, for example, persist from the time of acquisition through adult life, in much the same way as do memories acquired during childhood. So far as we know, no exact tests have been made of the retention of sentiments or other acquired complexes of affective elements in our experience, so that the variations in this group of mental functions cannot be compared, on the basis of exact data, with variations in the concomitant physicochemical processes. However, comparisons of memories and habits with the associated physicochemical processes can be made fairly decisive, so far as the method of concomitant variations is capable of yielding decisive results.

This comparison leaves little doubt as to the results, though the meaning thereof may be open to debate. Particular memories and habits often persist, with comparatively little loss of efficiency, for long periods of time after the numerically identical chemicals and energies associated with their acquisition have been displaced, and, in some cases, for periods of time during which the numerically identical chemicals and energies of the brain and nervous system must have been displaced over and over again. These facts show clearly that memories and habits are not completely dependent on the physicochemical process of the brain and nervous system, or on any *tertium quid,* of which the mental and the phyiscal processes are but correlative aspects.

Four possible rebuttals of this conclusion are open to physicochemicalists. First, they may challenge the validity of our assumptions respecting the time-rate by which proteins in the brain and nervous system are displaced, and others substituted for them. This line of attack would avail but little, however, even if it succeeded in rendering those assumptions questionable. For it would have to be shown, in addition, that the brain and nervous system are *not at all* subject to the processes of waste and repair characteristic of other living tissues. Such a proposition involves consequences so farreaching that, should it be established, the fundamental conceptions of physiology would have to be radically reconstructed.

Even the establishment of that hypothesis would not serve to rebut the conclusion supported by our analysis. For it would have been demonstrated at the same time that the proteins combined in the brain and nervous system are chemically inert, so that the psychological

[1] Bourdon, B., "Recherches sur l'habitude," *Année Psychologique,* Vol. VIII, 1901, pp. 327–340; summarized by Thorndike, E. L., *The Psychology of Learning,* pp. 318–320.

functions associated therewith would have to be ascribed to electrical phenomena, if the mechanistic conception of those functions was to be retained. But an interpretation of those functions in terms of electrical phenomena would involve entirely new conceptions of electricity and of energy in general. For one thing, we should have to picture the nervous system as a group of storage batteries endowed with the faculty of renewing themselves, in order to maintain their functional efficiency. Furthermore, precisely the same electrical energies would have to be retained, if the persistence of particular memories and habits was to be accounted for. But any such assumptions are, of course, insupportable. The only demonstrable source of the electrical energy found in nervous and other living tissues is the chemical changes in those tissues. And electrical phenomena associated with nervous functions involve the transformation of electrical into other forms of energy, since it is an axiom of physics that energetical phenomena always result in the transformation of at least a portion of the given energy into one or more unlike forms of energy. It is beyond doubt, therefore, (1) that the source of the electrical energy in nervous tissues is chemical action in those tissues, and (2) that this electrical energy, however derived, does not persist in the way required to account for the persistence of memory and habit functions.

These considerations would appear to overthrow that particularistic form of the physicochemical hypothesis which interprets consciousness in terms of potential energy. Any potential energy there may be in the brain and nervous system must be associated with the chemicals combined therein, and must therefore be displaced with somewhat the same rate of frequency as are the chemicals themselves. It could not serve, therefore, to account for the persistence of memories and habits. If a rebuttal of the sort above indicated should be preferred by advocates of this energistic conception, they would find themselves reduced in the end to invoking chemical action as the source of the potential energy in the brain and nervous system, whereupon the same consequences would follow. Moreover, the potential energy assumed by this hypothesis would have to do its work through a transformation into kinetic energy of some sort, and there are no grounds for the supposition that such kinetic energy would be transformed back into potential energy in such a way as to assure the persistence of particular habits and memories.

A second sort of rebuttal open to physicochemicalists would be to argue that, though the chemicals and energies combined in the brain and nervous system are constantly being replaced by other chemicals and energies, the structures of the system remain substantially the

same. It might be contended that memories and habits are identified with these structures and the corresponding functions, and not with the particular chemicals and energies associated with those structures and functions. This is the type of rebuttal which presumably would be undertaken by physicochemicalists of the organismic school.

Such a rebuttal can, we believe, be effectually demolished. The structures and functions appealed to cannot have, according to any physicochemical hypothesis, an existence apart from the chemicals and energies associated therewith, but would be the interrelations or systems of interactions of those chemicals and energies. When these chemicals and energies are displaced, therefore, the given structures and functions will be displaced at the same rate, though replaced by structural and functional elements of similar kinds. The point is that the structures and functions associated with numerically identical chemicals and energies will not themselves be numerically identical with the structures and functions associated with numerically different chemicals and energies, though similar thereto. But such numerical identity is requisite, on this hypothesis, to the complete correlation of neural structures and functions with particular memories and habits, since the latter *are* numerically identical. Physicochemicalists cannot appeal to such a numerical identity of neural structures and functions, consistently with their hypothesis.

A third sort of rebuttal would be to claim that the proteins and energies remaining in the brain and nervous system while the processes of waste and repair are taking place somehow impress the memories and habits identified with them on the newly formed proteins and energies which replace the old, so that memories and habits undergo waste and repair, day by day, in somewhat the same way as do the nervous tissues themselves. On this hypothesis, the old proteins would be said to educate the new ones in the functions to be subserved by them all.

Let us see what this hypothesis involves. The given memories and habits could not be identified with particular chemicals or energies, for memories and habits do not disappear and reappear in the same way as do the broken down and newly formed proteins of the nervous system. Moreover, it is quite certain that memories and habits are correlated with a large number of individual neurons, and hence with large numbers of proteins and the associated energies. What we should have then, according to this hypothesis, would be large numbers of proteins coöperating in the education or discipline of each newly formed protein so that it should serve effectively the specialized function of its predecessor. For ourselves, we can form no con-

ception of how this might be done. It is certain that any such process would be radically unlike anything we know of physical and chemical action. Until the physicochemicalist shows us, therefore, how it might be done, we are not justified in entertaining the hypothesis that it can be done.

Moreover, this hypothetical process, whatever it might be, would imply that the given habits and memories are constantly being destroyed and recreated anew, and by a process and time-rate quite dissimilar to the process and rate whereby they are established in the first instance. Should we assume that two series of processes so dissimilar might have the same outcome, it would be necessary, in addition, to invoke some sort of *deus ex machina* capable of making them turn out just the same.

The fourth sort of rebuttal open to the physicochemicalists is really a variant of that just considered. According to this rebuttal, numerically identical memories and habits do not persist any more than do the numerically identical chemicals and energies correlated with them, but the *qualitative* identities of all these persist, including the qualitative identity of the organization of these chemicals and energies. The qualitative identity of early habits and memories with later ones would, according to this hypothesis, fully explain the common belief that the earlier and later ones are numerically identical. This belief is quite similar to the common-sense belief that our bodies are numerically identical throughout our lives, and it might be quite as erroneous as the latter belief now appears to be. More generally, according to this hypothesis, the identity of the psychical or spiritual self is a qualitative, not a numerical, one.

This hypothesis is clearly open to one of the criticisms directed against the previous hypothesis, assuming, as it does, that two quite *dissimilar* series of physical events lead to quite *similar* results, and necessitating, as does the other hypothesis, the postulation of some sort of *deus ex machina* capable of determining such equivalence of results. The justice of this criticism would be vindicated, as against the possible rejoinder (drawn from analogy) that unlike series of chemical reactions produce the same compounds, by pointing out that the *elementary* factors which enter into the two unlike series of events yielding, by hypothesis, similar habits and memories, are themselves, in large part, of quite *different sorts,* as will appear from a comparison of external stimuli, sensory impulses, etc., entering into one series, with metabolic and allied processes entering into the other (hypothetical) series.

The foregoing discussion reveals, it would appear, insuperable diffi-

culties in the way of accounting for individual memories and habits on any form of the physicochemical hypothesis. Difficulties of this sort at least are not encountered by a theory such as ours, that does not demand a perfect correlation between variations of mental and of the concomitant physicochemical processes, and particularly between variations of numerically identical members of the two categories.

Although, so far as we know, the retention of other acquired mental functions has not been subjected to exact measurements, it seems feasible to deduce from the preceding analysis a refutation of mechanistic interpretations of such functions. The particularized functions of other types which appear from their intrinsic nature to be capable of retention for considerable periods of time include sentiments, the products of cognitive processes (meanings, beliefs, ideas, problems, etc.), desires, interests, volitions and possibly some other classes of acquired mental complexes. Analysis of such complexes would show that habits and memories are fundamental components thereof, since the individual's stock of such complexes would represent the outcome of his past experiences, and this of course would be described largely in terms of habit and memory. This being true, monistic physicochemical interpretations of these complexes would be shown, by the method of concomitant variations, to be untenable.[1]

One could compare, by the method of concomitant variations, the various *types* of mental processes with the associated *types* of physicochemical processes, and while some sort of correlation between the two could always be identified—since mental processes are always dependent on the nervous system and, ultimately, on stimuli from the physical environment—, the *degree* of correlation would probably in no case be such as to warrant an exclusively physicochemical interpretation of the given mental processes.

Such a comparison has to some extent been worked out by other investigators. Ladd and Woodworth have shown very clearly that cerebral and mental developments do not parallel one another in any exact sense. "At certain epochs of life," say these authors, "the evolution of the brain seems to stand far in advance of the mind; at others, the mind appears to have overtaken and passed by the stage reached by its physical substratum." The formation of so-called

[1] It is not denied, but on the contrary affirmed, that physical (sub-physical) factors enter into the genesis of all such psychic complexes. Our contention throughout is that such complexes are not *completely* identified with factors of this class.

The foregoing discussion obviously supports our theory that the hereditary endowment of the organism includes sub-mental factors which are distinct from the associated sub-physical factors, since mental functions are an expression of hereditary sub-mental factors, together with factors of other categories.

dynamical associations among the molecules of the nervous system does not, in the opinion of these authors, adequately account for mental development. In short, "so far as we really *know* anything about the development of both brain and mind, we are compelled to say that the latter, when once started by the sensations furnished through excitation of the former, proceeds to unfold its activities with a rapidity and in an order for which no adequate physical causes can be assigned." [1]

Messrs. Ladd and Woodworth point out also that the effects of lesions in the brain of man or the extirpation of brain substance in animals do not favor any theory which completely identifiies nervous and mental functions; and that, on the other hand, serious interruptions of mental development occur for which no adequate cause in cerebral developments is discoverable.[2]

McDougall has pointed out, in a series of chapters, that the correlations between mental states of various sorts and the concomitant brain processes are quite incomplete.[3] The unitary resultant, in consciousness, of the effects produced by different sensory stimuli simultaneously applied to the sense organs cannot be the outcome of a physiological or physical fusion, according to McDougall, because the nervous processes involved do not undergo any such fusion. This resultant must therefore be primarily due to psychical fusion. Similarly, feeling-tone pertains to consciousness as a whole, whereas the multiplicity of the brain processes which play some part in determining the nature and intensity of a feeling-tone are not compounded into a single physical resultant which could account for this unitary character of the given feeling-tone. Aesthetic pleasure, likewise, arises from the synthesis of sensations into an object of a higher kind, but such synthetic activity, as in the other cases, has no assignable physical correlate. Meanings, in turn, are psychical wholes depending on synthetic activity of the same general sort, and therefore do not have as physical correlates in the brain corresponding unitary wholes. The same general analysis applies to memory also, for meaning coöperates with habit in the genesis and reproduction of memories, and meaning is the more effective of the two as a condition of mnemonic reproductions.

We shall not take it on ourselves to appraise McDougall's arguments on these questions, but will remark only that his principal contentions appear to be well grounded; and that, if it be true that the physical

[1] Ladd, G. T., and Woodworth, R. S., *Elements of Physiological Psychology*, pp. 657–661; Charles Scribner's Sons, publishers.

[2] *Ibid.*, p. 663.

[3] *Body and Mind,* Chaps. XXI–XXIV.

correlates of mental states do not correspond in detail to the latter, the concomitant variations of the two series are not completely correlated either.

We have thus arrived at another conclusion significant for our inquiry, namely, that correlations between *types* of mental processes and the associated *types* of physicochemical processes are by no means complete, and, in important details, are wanting altogether.

This conclusion can, we think, be given a more generalized form. Unless the results attained through psychological research are fallacious to an astonishing degree, the laws of mental life are not physical and chemical laws. Only the more extreme behaviorists would claim that this is so. But it is fair to say that they attempt to vindicate this claim by flatly denying or ignoring a large part of the facts to be accounted for, and by surreptitiously extending the scope of physical and chemical laws to cover the remainder, involving the addition of new laws or principles that would not be recognized by physicists and chemists themselves as falling at all within their fields.

We cannot for lack of space undertake to justify this general conclusion in detail. But we should like to present a brief comparison of certain types of cognitive processes and their probable concomitants in the nervous system; and also, to take an instance which would doubtless be regarded as more legitimate by the behaviorists, a comparison of human behavior in certain of its aspects, with the physical concomitants of this behavior.

Cognitive processes are guided or determined in large measure by logical principles, of which there are no assignable physical correlates. The processes involved in judgment, the genesis of concepts, the analysis of propositions, etc., have no conceivable parallels in the physicochemical processes concomitant therewith. There is nothing discoverable in brain processes which is at all similar to the formulation of a law or principle, the classification of individuals into species and genera, or the discrimination of sensory qualities. We cannot identify or conceive brain processes paralleling the abstraction of homogeneous qualities, whether pertaining to material bodies, as blueness, hardness, bitterness, etc., or to the relationships between members of a social group, as honesty, justice, responsibility and the like. Should we attempt to conceive such mental processes in terms of material processes, we would have to try to think of them as the telescoping, fusion and interpenetration of material bodies; as the occupancy of the same space by different bodies; as an infinitely complex system of selective attractions and repulsions between bodies; as interactions between mere aspects of material bodies. The brain

correlates of mental processes cannot be imagined to behave in any such ways as these. We are forced to conclude, by a parity of reasoning, that concomitant variations in the two series cannot be completely correlated.

Let us now compare certain significant features of human behavior with the concomitant physicochemical processes. Such processes, whether or no they have their locus in the brain and nervous system, occur only in the present. They know nothing of the changing seasons, of a coming eclipse, of recurring sunsets, or even of the next moment. But the human organism acts most of the time with reference to anticipated future events. It lays in coal for the winter, plans its vacation for the coming summer, prepares a lecture for the next day, orders groceries for the midday meal, makes arrangements for a year of travel abroad. It acts with reference to time yet to be, and to places more or less distant from that where it happens to be at the moment, and with which it is not related, in a physically effective way. While its actions are in the present, they face the future and help to make that future—and the more or less distant future at that—what it will turn out to be. Anticipations of future events thus partially determine the order and organization of our present activity. The future shapes our present activity, in part, because we have learned from experience that it fulfills many of the promises it holds out to us. We find that the groceries ordered for dinner do often provide a palatable and digestible meal, that we do generally deliver our lectures according to schedule, that our vacations do sometimes justify our hopes of them, and so on.

Moreover, the human organism investigates the past. It does so, of course, by dealing with records of various sorts existing in the present, but yet it acts with reference to a past time, attempting to discover the nature and order of the events that actually occurred in the past. But the temporal references of masses and their motions pertain to present, not to past, time. While the past motions of purely material bodies will have determined their present positions, their present motions are not retrospective, as is so much of the human organism's behavior. The chemicals and energies combined in the brain and nervous system cannot account, therefore, for the past and future time references or the remote space references of the human organism's behavior. The mental processes to which those references must be ascribed cannot therefore be completely correlated with the associated physicochemical processes of the brain and nervous system, nor can concomitant variations in the two series be so correlated.

We may now touch on another aspect of the correlations between

types of mental processes and the types of concomitant physicochemical processes in the brain and nervous system. Our knowledge of bio-physics and bio-chemistry is of course far from exhaustive, and there may be many sorts of physical and chemical processes in brain and nerve tissues of which we are now totally ignorant. But what is known of the subject affords no justification for any belief that the types of those processes vary in strict correlation with the types of the concomitant mental processes. So far as we are able to judge, the distinct sorts of physical and chemical processes in those tissues are far less numerous than are the distinct types of mental processes. How meager must even an exhaustive inventory of the types of physical and chemical processes in those tissues be, compared with the richness of mental processes represented (for example) by our art and literature, our science and philosophy, our complex customs and institutions! A detailed comparison of the two series cannot of course be carried out at the present time, because of our limited knowledge; but the point here is that what we do know affords no grounds for the speculation that when this comparison shall be possible, it will reveal anything like a perfect correlation between types of mental and of the concomitant physicochemical processes.

Moreover, little or no justification is afforded by present knowledge for the supposition that, in the development of the individual, the types of physicochemical processes in the brain and nervous system change in ways at all comparable with changes in the concomitant types of mental processes. The chemistry of neural processes must be much the same for different periods in the life of the individual, but no parallel observation would hold for the mental history of the individual. This fact is so obvious that specific illustrations may be dispensed with. Although, as stated, detailed comparisons are not practicable at the present time, the comparisons which our general knowledge does permit appear to justify fully the conclusion that the correlations between typical processes in the two series are of such an order as to lend no support to the physicochemical interpretation of mental processes.

In establishing the incompleteness of the correlations that obtain between mental and the concomitant physicochemical processes, we have been obliged to rely, in large part, on deductions from facts which do not lend themselves directly to such a purpose, and, in other cases, on an admittedly fragmentary knowledge of the facts involved in the comparisons undertaken. We believe, however, that our conclusions have not been pressed beyond the point justified by the known facts and the inferences adequately supported thereby.

These conclusions, it should be noted, are substantially equivalent to conclusions attained in a previous chapter by the employment of quite different methods. This fact would appear to supply additional testimony as to the correctness of these conclusions.

Correlations Between Cultural and the Concomitant Physicochemical Phenomena

We shall now undertake a comparison of cognitive processes affecting cultural evolution, and the physicochemical processes concomitant therewith. Direct comparisons are here possible, and capable of yielding clearcut conclusions. For this reason the matter will be presented at some length.

We do not need to insist on the importance of cultural factors in human life. They determine a large part of the differences between primitive and modern societies, between any two societies, in fact, which are separated in time or space. The causative efficacy of these cultural factors is beyond question. We only have to consider the differences in the physical and social environments of, say, an Indian village and a European metropolis to grasp at once the importance of the causal rôle played by this group of factors. More particularly, they are a most important group of factors in the arrangement of bodies in space, as the example of the Indian village and the European metropolis clearly demonstrates.

Their influence on the fortunes of the human species, viewed biologically, is equally profound and farreaching. Through them the substances taken into the body, particularly food substances, have been greatly changed in character. They have profoundly modified the adjustments of the human organism to temperature, rainfall and other climatic conditions. They have virtually revolutionized the conditions of survival for the human species. The factors determining survival and extinction of racial stocks are now profoundly different from what they were ten thousand, or one thousand, years ago. Great inequalities in the incidence of these factors have resulted from the operation of factors coming within the same category, as is illustrated by the effects of the present system of wealth distribution, the celibacy of priests and nuns, the humanitarian care of physical and mental defectives, and so on.

The effect of these factors on *individual* behavior is equally significant. Habit has been called second nature, and that is not too strong a term whereby to denote the influence of habit in individual behavior. Yet most habits of the individual are derived from the cultural factors

by which he is conditioned. Cultural factors of the given society become habits in its individual members, and indeed play their part only by being thus translated into individual action.

It is true that all these factors are to be referred, for their ultimate explanation, to hereditary characters of the organism, but the cultural factors themselves are largely independent of individual human organisms, although dependent on the latter for their origin and influence. We have in these cultural factors a group of independent variables, so to speak, which is of coördinate importance in human life—whether regarded biologically or otherwise—with the hereditary traits of the organism and the physical environment, two other groups of independent variables with which the first group is generally associated. Proof of the independent variability of these groups of factors is supplied by the fact that any human organism could develop in any physical environment and under any complex of cultural factors which are adapted to the human species generally. There would of course be temporal and other restrictions on this possibility, but these do not diminish the significance of the general statement.

It is clear that biological theory must adjust itself to this group of factors, and that any theory of life which is to withstand criticism must supply an interpretation of these factors which does justice to the biological rôle which they indubitably play. Culture plays a smaller part in the life of other animal species, of course, although evidence is not wanting that it plays a rôle of considerable importance among a number of the lower species. The undoubted fact, however, that it plays a rôle of the greatest importance both in the arrangement of material bodies on the earth's surface and in the evolutionary history of at least one animal species proves that it has a significance for biology and for science in general which is not to be overlooked.

Let us see whether physicochemical hypotheses can square themselves with this body of facts. Cultural elements have their origin in the past thought and action of human individuals, some of them still living, but most of them long dead. Thought is the most significant phase of this process, and it will simplify the problem to limit our consideration to the rôle which thought plays in this connection.

We may call the matter and energy involved in thinking the material substratum of thought, and this without minimizing the dependence of thought on its concomitant material processes. Now, it cannot be denied that innumerable thought processes, or at least the products thereof, are potent in human life and evolution, biologically regarded, long after the material substrata of those thought processes have

disintegrated. The thoughts of a host of thinkers have been potent in human life and evolution for centuries since the configurations of matter and energy constituting the material substrata of those thoughts underwent dissolution. Whatever theory one may accept regarding the relation of thought to matter, it is obvious that the matter and energy associated with those thought processes have not continued to produce the same thoughts, even though some of that matter and energy may have gone into the brains of other thinkers who have reproduced, through learning processes, those same thoughts, or reacted to them in other ways.

Yet the thoughts of those thinkers live on and many of them, for anything we can see to the contrary, are immortal. Thought thus appears to be independent, to a marked degree, of its material substratum. Certainly thoughts themselves, as distinguished from their material substrata, have exerted an enormous influence on human life and evolution, and even, as we have seen, on the inorganic world. The thoughts of Plato, Aristotle, Galileo, Newton, Helmholtz, Darwin and other thinkers, the combination and interaction of which have produced modern science and placed it at the service of mankind, exert a causative rôle out of all proportion to any which could be claimed for the configurations of matter and energy associated with the mental processes of these thinkers.

There is no possibility, on the showing made thus far, of equating the two groups of factors considered, in respect to the causal rôles which they play in both the organic and the inorganic worlds. Not only do the thoughts play causal rôles out of all proportion to any which could be assigned their material substrata, but the thoughts play a causal rôle for incomparably longer periods of time. For if there is no break in the cultural history in which any given thought or system of thoughts finds its place, that thought or that system *never* loses its causative efficacy, even though, as normally happens, it is continually modified and taken up into new thought syntheses.

Moreover, particular thoughts commonly, if not always, play their causative rôles *after* the material processes associated with them have been terminated. For the communication of a useable thought—the first step whereby it approaches social and biological efficaciousness—must, in the nature of the case, come after the series of physicochemical processes associated with the origination of the thought has been completed. And the major influence exerted by useful thoughts generally comes after the death of those who originated those thoughts, and therefore after the material substrata of those thoughts have

suffered dissolution. We may conclude, therefore, that no strict correlation can be established between thought and physicochemical processes in the brain, when these are regarded as causative agencies.

Some plausible objections might be urged against this line of reasoning. It might be objected, in the first place, that the social or biological effects of a given thought will depend on the number of persons (or nervous systems) to whom (or which) it is communicated, and on other material agencies with which the thought becomes associated (as in the industrial applications of a chemical discovery, for example). In other words, a thought is causally effective only and in proportion as it becomes associated with material factors.

These claims are true, but they scarcely touch the validity of the previous argument. For, first, the thought itself is the primary causal factor. It is effective only as it is correlated with the brains of human beings, but it is the *prior* thought which is thus related, and the *directive* or *organizatory* influence of the thought which modifies behavior, or determines the order of material changes in the environment. The thought was on the scene first, if we may so speak. The thoughts of Plato, Galileo or Newton were active in the world long before any person now living was born in the world. The thought is therefore the first member of the causal series in which it is operative.

Secondly, it is the thoughts of Plato, Galileo, etc., in the most literal sense of the term which are active. Later actors on the human stage *learn* these thoughts, and they may correct and modify them; and the thoughts are effective at any given time only as they are learned and translated into action. But the thoughts existed prior to the learning and utilization thereof, and cannot be interpreted, causally, in terms of the brain processes involved in the learning and utilization. We seem justified in affirming, therefore, that the thoughts of past thinkers in a most literal sense of the term continue to play a causal rôle in the world to-day. The thoughts for all we can see *become* quite independent of the original thinkers themselves and of the material substrata of those thinkers' mental processes.

It may be objected, also, that thoughts are communicated by material symbols, that there is a material concomitant of every process in the origination, communication and utilization of thought, and that it is the material factors involved which are real and efficacious in a causal sense, and not what we are pleased to call thoughts. It might be claimed that causal series in which thoughts find a place could be traced back to the thinkers that originated and communicated these

thoughts, and that every member of such causal series embraces material processes which give that member of the series its causal efficiency. Let us examine this claim.

The brain processes involved in thinking cannot as such be transmitted to other organisms. And the material processes whereby thought *is* communicated are of a totally different character from the brain processes involved in thinking, although both fall, broadly speaking, within the general category of bodies and their movements. Now, thought must, on physicochemical hypotheses, be identified with one or the other of these series of material processes, or with them both. The acceptance of any one of these alternatives by the physicochemicalist leads to impossible consequences. But they are the only alternatives open to him. On his assumptions, either (1) thought is wholly identified with brain (and perhaps other bodily) processes, in which case it cannot be communicated; or (2) it is wholly identified with the material processes employed in its communication, in which case the brain has nothing to do with it; or (3) it is identified, indifferently, with the brain processes or with the material processes employed in its communication, in which case the same thoughts are at different times and even at the same time (the time of communication) wholly different things, or correlates, aspects or epiphenomena of wholly different things; or (4) thought is identified both with the brain processes and with the material processes employed in its communication, in which case thinking cannot occur without its communication. Moreover, by any one of the last three alternatives, a single thought could be a thousand different things at the same time, since its nature could be a function of any system of symbols employed in its communication. Additional comment seems unnecessary. Obviously thought cannot be exclusively a function either of the brain (and other bodily) processes involved therein, or of the symbols employed in its communication, or of the brain processes and symbols combined.

The argument from the diversity of the symbols employed in the communication of thought may be put in a different form. As has already been intimated, thought is *indifferent* to the symbols employed in its communication, provided an adequate *medium* or *instrument* of communication is available. Any language may be employed, the communication may be oral or written, the sensory stimuli employed may be auditory, visual or even tactile in character. It could scarcely be maintained, therefore, that air waves, light vibrations or pressure stimuli employed in the communication of thought are significant members of the causal series into which thought enters.

If one maintains so extravagant a proposition, it must be admitted

that an indefinite number of material factors (air waves, light vibrations and tactile stimuli employed in the diverse systems of communication in every developed language) may produce the same effects. These systems of symbols might theoretically be infinite in number, and no two of them alike, if we consider the differentiated tonal qualities of oral communication by different speakers, the distinctive characteristics of individual handwritings, the diverse combinations of typographical styles, different qualities of print paper, etc. Now, we could scarcely maintain, on accepted logical principles, that it is the factor or group of factors which need never be the same in a causal series where the effect is always the same, that is responsible for the effect, and that it is the factor which is always the same that has nothing to do with the effect. If that be so, we shall have to revise all our conceptions of cause and effect, give up indeed our postulate of law or of causal series in nature, and admit that science is impossible. The fact that the same idea or thought is not always followed by the same effect does not affect this argument. The given thought or idea will always produce the same effect, provided all other conditions (not counting the symbols employed) are the same.

The most superficial introspection should convince anyone of the truth of this contention. When we are reading Plato's *Dialogues* or Newton's *Principia,* for example, we are dealing, not with the symbols whereby these thoughts are communicated—that is, if we are an adept in using the system of symbols employed—, we are grappling with the *thoughts* of Plato or of Newton in the most literal sense of the term. The same analysis applies to any oral or written communication, although we may be interested in the style or other incidental features of communications to which we are attentive. Thought is as indifferent to its symbols as a liquid is to its container, and it would be as reasonable to claim that a glass bottle containing hydrochloric acid, for example, is to be credited with the properties of that chemical as to claim that air waves, light vibrations, etc., whereby thought is communicated are to be credited with the potencies which thought appears to possess.

This indifference of thought to its symbols is emphasized, and at the same time accounted for, by the fact that meanings are assigned to symbols by convention, so that the symbols will mean anything which the given language group may agree that they shall mean. Hence, given social groups developing in comparative isolation from one another, the diversity of the symbols employed for the same meanings naturally follows. We may observe, incidentally, that physical phenomena such as processes in the brain, or certain sorts of black

marks on white paper, do not choose, by convention or otherwise, to associate themselves together in any way they please, and in infinitely diverse ways at that. The arbitrary ascription of meanings to symbols must be attributed, not to brain processes or to material processes in a communicating medium, but to thought processes associated with those phenomena.

It is obvious that, for any given system of symbols, the thoughts to be expressed determine the selection and the order of the symbols employed, and are not determined by the latter. The thought to be expressed determines the particular symbols to be employed, for any and all systems of symbols. The absurdity of any proposition that the symbols are the factors back of changes apparently produced by thought could be at once exposed by pointing out that this proposition implies that the symbols employed in expressing the thoughts of Jesus or Socrates in the French language or in Esperanto, for example, are to be credited with the effects produced on one through a French or Esperanto translation of the sayings of Jesus or Socrates, when neither language had been evolved when Jesus and Socrates lived. The thoughts of Jesus and Socrates were on the scene before the French or Esperanto (or any other modern) system of symbols was evolved, and it would be absurd to interpret causally the thoughts of Jesus or Socrates in terms of the symbols which have arisen since those thoughts were first uttered. The thoughts are primary, in the causal sense, and the symbols are only instrumental.

We may add, as another item in the comparison of thought processes with the associated symbols and brain processes, that the historical development of the two series exhibits, at the most, but very incomplete parallels. The thought processes issue in myth and dogma, art and literature, science and philosophy, ideals and traditions, all undergrowing cumulative growth or development; while brains and nervous systems have been of much the same sort throughout this history. Nor have the systems of symbols serving as vehicles for the communication of these thought-products changed in the same ways as have the thought-products themselves. A scientific discovery or the composition of an opera, for example, does not necessarily involve the invention of a new symbol. New symbols are of course invented in connection with creative intellectual activity, but the results of intellectual activity at any given time are communicated for the most part by means of preëxisting symbols. And, to repeat a previous point, the results of intellectual activity always determine the choice and arrangement of the symbols employed in their communication. The symbols are not determinative of the intellectual activity, for the

letters and words of a given language do not themselves produce poetry or other intellectual creations in that language.

Nor is the development of science, philosophy or literature correlated in any strict sense with the growth of the human species in numbers. There may be a vigorous intellectual activity in a comparatively small social group, and but little original intellectual activity in a populous social group. Knowledge is not destroyed in the same sense as human organisms are. Knowledge goes on growing day after day, and generation after generation, whereas human organisms die and are succeeded by other organisms of the same species. The process of cultural continuity in human society is altogether different from the process of biological continuity underlying human society. Owing to this difference, the per capita amount of scientific knowledge in the world to-day, if we may put the matter that way, is greater than it was a generation ago, and very much greater than it was a thousand years ago.

As a final item in the comparison, we may point out that matter and energy do not increase in amount, whereas knowledge does. Matter and energy are conserved, but knowledge is cumulative. Thought is creative, for it is constantly adding to knowledge; but matter and energy are not creative, in the same sense of the term. The latter may be combined into new aggregates, but they do not add to the amounts of matter and energy in the universe. Thought does add to the amount of knowledge.

A similar analysis could be worked out for other aspects of cultural evolution. Customs, institutions, arts, traditions and other cultural factors are to be interpreted as modes of behavior and of sentiment as well as modes of thought. Since modes of thought originating in the past are essential elements of all such cultural factors, none of them could be interpreted exclusively in terms of physicochemical processes. Moreover, when regarded from the standpoint of the bodily movements or types of sentiment partially determined by, and constitutive of, these cultural factors, the latter could not be interpreted in terms of physicochemical processes alone. The complex system of bodily movements partially determined by, and at the same time constitutive of, our Federal Constitution, for example, could not be wholly accounted for in terms of the discrete bodies and motions that enter into this complex of movements. The transmission of the cultural factors constitutive of the Federal Constitution must be regarded, from this point of view, as a transmission, not of numerically identical bodies and energies, but rather of complex *modes* of motion which may be correlated indifferently with any bodies and

energies of certain specific *sorts* combined in certain specific *ways*—the sorts and combinations peculiar to human organisms capable of entering into our constitutional system. The motions of purely material bodies involved in this system could scarcely determine the transmission of *modes* of motion of this sort. Physics and chemistry have no concepts under which any such modes of motion could be subsumed.

Sentiments likewise are inexplicable on any physicochemical theory of the human organism and its experience. Even granting that the specific types of feeling and emotion combining to produce sentiments current in a given social group could be interpreted in physicochemical terms, it would be these specific affective elements that were transmitted by physicochemical processes, and not the complex sentiments into which they are developed in the given social environment. For the latter are developed in the individual through learning, imitation and other assimilative processes, similar in a general way to modes of thought and behavior already considered, and, like them, inexplicable in terms of the correlative physicochemical processes.

Cognitive or thought processes are really the significant feature of all these cultural factors, including their origination, transmission, assimilation and continuous modification, and these cognitive processes largely account for the complex modes of feeling and behavior partially constituting those factors. Human behavior is so widely differentiated from the behavior of other animal species, because human beings are capable of enormously more complex types of thought than other animals, and this serves both to make the behavior of the human organism, when abstracted from its cultural background, widely different from that of other animals, and to make possible the communication to and assimilation by other organisms, of modes of behavior initiated in the first instance by types of thought peculiar to the human species. The development of the sentiments parallels the development of thought and behavior, whether regarded from the standpoint of the individual or of the social group; and the significant factor in this process is the type of thought peculiar to our species and differentiating it, all along the line, from other animal species. This analysis by no means implies that instinctive impulses and the affective experiences associated therewith are not potent in human behavior. On the contrary, they are of coördinate importance with the cognitive processes themselves, and indeed largely determine the objects or situations with which the latter are concerned, and to which the cultural factors made possible by those cognitive processes have reference.

Summing up, we may say that the only thing proved by the correla-

tion which subsists between material and cultural factors is that the latter are interdependent, and in all their manifestations, with material factors. Material processes do not wholly account for cultural factors and their causal efficacy, as our analysis has demonstrated. And if material processes do not account for cultural factors and their causal efficacy, they do not wholly account for the human organism and its properties, since those factors not only play an important rôle in human life and evolution, biologically regarded, but their origin depends on the activities of the human organism. We have here not only a case of incomplete correspondence between physicochemical processes in the organism, and organismic processes in their entirety, but also a most important group of factors in the life of one animal species which cannot possibly be equated with physicochemical factors. It is difficult to see how the physicochemical theory of life can maintain itself when squarely confronted with this body of facts.

The ontological status of these cultural factors, especially ideas transmitted by the past, may be briefly considered. Perhaps the simplest conception would be to regard ideas transmitted to us from the past as communications of all the thinkers who contributed to the development of those ideas. This would mean that those thinkers had achieved a species of immortality, and continued to live and work in the world long after their bodies suffered dissolution. Or it is possible to suppose that their ideas became wholly dissociated from their personalities, intellectually regarded, and lead a semi-independent career of their own. Ideas long submerged and unknown to any human beings living during that period may be later brought to light through the discovery of written records by which they are symbolized, and thus made potent in human events once more. Ideas thus seem susceptible of a process analogous to canning or cold storage, by which they may be wholly dissociated from any human beings, living or dead, but yet just as available for use, when discovered, as are material commodities preserved in anticipation of future need. The cold storage process is of course widely employed in all types of intellectual activity to-day.

It is not so important, for our purposes, to decide between these alternatives, as to point out the dissimilarity of the processes whereby bodies and their motions, on the one hand, and thought and other cultural factors, on the other hand, are transmitted from one period of time to another. The two alternative hypotheses respecting the ontological status of the latter will serve to emphasize this contrast.[1]

[1] The relation of culture to other categories of entities is systematically discussed elsewhere in the text. *Supra*, pp. 293 ff.

We have not in the foregoing discussion treated explicitly the various epiphenomenalistic, parallelistic and two-aspect theories of psychophysical relationships, combined in various ways with mechanistic conceptions of life. It should be obvious that our analysis applies to these several types of theory, as they all maintain that mental processes are completely correlated with the concomitant physicochemical processes. If our analysis has yielded correct conclusions, therefore, it constitutes a refutation of theories coming under these various types.

Nor have we differentiated, throughout the discussions of the present chapter, between the various elementalist, organismic and particularistic versions of the physicochemical conception, upon all of which the discussions have a bearing. Clearly, however, the analysis, if it demonstrates incomplete correlations between the physicochemical processes of the organism and its various morphological, functional and mental processes, applies with destructive force to the several types of the physicochemical conception, for all these assume that a detailed correlation obtains between the two series of phenomena.

We might, in conclusion, stress the significance of the congruity of the results attained in this chapter, by the method of concomitant variations, with the results attained in previous chapters by quite different methods. We have before called attention to this fact, and we wish here only to emphasize again the point that the substantial equivalence of the conclusions yielded by independent lines of analysis, especially in the case of problems so complex, would appear to constitute specially weighty testimony as to the validity of these results.

CHAPTER XII

OBJECTIONS TO THE ANALYSIS OF MECHANISTIC CONCEPTIONS OF LIFE

We shall conclude our analysis of mechanistic conceptions of life by anticipating and answering certain objections which are likely to be urged against the type of theory supported by this analysis. We need only refer to possible objections which have been answered, by implication, in the first part of the analysis, objections which would elevate advantages of the mechanistic hypothesis there termed adventitious into advantages of an evidential or logical order. The accessibility of matter and energy to scientific inquiry, the quantitative character of the physical and chemical sciences, the ubiquity of matter and energy, particularly their intimate association with every form of life known to us, the undoubted successes (of their kind) which have attended the physical and chemical analysis of vital phenomena, the opposition of mechanistic philosophies to supernaturalism in science—these facts were shown to be quite irrelevant to the merits of the mechanistic hypothesis when confronted by non-mechanistic hypotheses admitting all these facts and all the inferences legitimately to be drawn from them.

Influence of Vitalistic Conceptions on the Biological Sciences

The first objection which we shall consider is that the assumption of non-material factors in the organic will confuse the sciences dealing with vital phenomena and, indeed, compel the reorganization of those sciences; and that scientific investigation of life phenomena will be rendered more difficult by the assumption of unanalyzed, and perhaps unanalyzable, factors in nature of this type. The truth of the first contention may be conceded. The biological sciences will be confused for a time by the assumption of non-material factors in the organic; but if there *are* such factors in this domain, it is of course necessary to recognize and deal with them, if a genuine science of living nature is to be established. Biology is already in a state of considerable confusion, and we should only be adding to the confusion, or substituting

one type of confusion for another, if we admitted the existence of non-material factors in the organism. And if there are such factors, an indispensable condition of the clarification of biological problems is to admit their existence and deal with them accordingly.

Such an admission will not increase the difficulties of biological research, except perhaps temporarily, for biology will thereby be exempted from the impossible task of explaining vital phenomena in terms of physical and chemical factors, when other groups of factors are really operative therein. Biological research will be rendered still less difficult when organizatory, mental and other non-material factors in life phenomena are more thoroughly analyzed. The inorganic sciences have been made less difficult and progress therein greatly facilitated by the demonstration of two classes of inorganic factors in nature—matter and energy—not to mention categories of factors less firmly established. The demonstration of non-material factors in the organic should have a similar influence on the biological sciences, if there are such factors in this domain of nature.

There is in fact a movement in the physical sciences at the present time which is quite analogous to the vitalistic movement in biology. Relationships in the inorganic have long been recognized which could not be explained on established chemical and mechanical principles. All those relationships which are subsumed under the so-called periodic law of the elements present a case in point. Although these curious relationships pointed to some underlying principle or cause, no progress could be made toward the discovery thereof by chemical and physical analyses of the usual type. Light on this problem eventually came from researches in radioactivity, and it now seems fairly certain that the relationships referred to may be accounted for in terms of elementary factors demonstrated by those researches. Indeed, there seems to be a distinct possibility that all our ideas of matter and energy will be revised in the light of the new discoveries. These are all examples of the necessity so well stated by Driesch that "something new and elemental must always be introduced whenever what is known of other elemental facts is proved to be unable to explain the facts in a new field of investigation." [1] There is evidently now the same logical necessity to assume the existence of non-material factors in nature, for vital phenomena have shown themselves refractory to an interpretation in terms of material factors alone. It might be suggested, by way of emphasizing the significance of the analogies presented, that non-material factors in vital phenomena may stand in somewhat the same relation to chemicals and energies in the organism as do molecular

[1] *The Science and Philosophy of the Organism,* Vol. I, p. 142.

structures to atoms, and atomic structures to the electronic components of atoms.

Alleged Sterility of Vitalistic Conceptions

It has been charged also that the assumption of non-material factors in vital phenomena is scientifically sterile, that it is incapable either of guiding biological researches or of being itself experimentally tested.[1] This objection goes with the claim that only physicochemical processes in the organism can be the subject of observation and experiment, and only physicochemical conceptions of life can be tested by these methods.[2]

Let us examine this objection. For one thing it implies, without explicitly stating, a criterion as to the validity of scientific hypotheses which is certainly open to question. This criterion is to the effect that a hypothesis concerning nature which cannot be experimentally tested must be adjudged a spurious one. The objection in question really goes further than that, for it implies that any such hypothesis, to be worthy of scientific investigation, must be susceptible of being tested by *present* methods of experimentation, or at least by present *types* of experimentation.

Both these implications of the objection under consideration must be rejected as unsound. The doctrine that only experimental investigation (or observation) can determine the validity of a hypothesis respecting natural phenomena is refuted by the established results of all those branches of science which employ non-experimental methods. These really include, in the last analysis, *all* departments of science, for they all utilize non-experimental methods, though these may be and are employed in relation to experimental and other observational data. These non-experimental methods could all be brought under the general category of logical or inferential analysis; being of this character, they are employed in the investigation of all complex scientific questions, and, indeed, all scientific questions, for any problem properly designated scientific is one to which an answer is not yielded by any amount of *unanalyzed* data. The technique of experimental investiga-

[1] Haldane, J. S., *Mechanism, Life and Personality,* pp. 61–65.

[2] Professor Guyer writes in a private letter, which we are allowed to quote: "Vitalism is like the measles, of course, you either have an attack or you do not. I think that most biologists have no fight to make against the idea; and in fact many of them incline towards some form of vitalism, but they work mechanistically as a practical matter because they get returns from such work, and but little from entelechies, etc. They feel that there is danger in using such labels, that we are likely to accept them instead of real explanations and that in consequence experimental investigation is likely to lag."

tion is indeed a technique of systematic analysis, many phases of which are remote from the bare experimental data requisite to a decision on the problems treated by this procedure. The nature of the given problem and the available technique of experimental analysis themselves determine the character of the data to be sought by observation or experiment. Some problems require a less extensive employment of inferential analysis than other problems, but many scientific problems cannot be solved without a sustained and difficult analysis of this type. Such are the problems raised by both vitalistic and mechanistic hypotheses.

These problems are not peculiar in this respect. The demonstration of the conservation of energy, the establishment of the theory of descent, the formulation of the law of gravitation, and the distinction between hereditary and cultural factors in social evolution all involved the same type of analysis. These and other similar investigations did not involve any neglect of observational or experimental data, but represented an extensive and complicated analysis thereof. The work of Driesch and other experimental biologists in support of the vitalistic hypothesis exhibits the same scrupulous regard for observational and experimental data as does that of mechanistic biologists in support of their hypotheses. They have simply carried the analysis of the relevant data much further than have the mechanists, and have not refused to accept the conclusions supported by such analysis. We may say, then, that the objection under consideration, so far as examined, evidently rests upon a misapprehension of the nature of scientific investigation, and, more particularly, of the part necessarily played therein by logical or inferential analysis.

The objection in question also implies, though it does not explicitly state, that hypotheses respecting nature, if they are to have any value, must be susceptible of test by present experimental methods or at least by present types of such methods. Some vitalistic hypotheses at least really meet this test, as the work of Driesch and others shows. They are utilizing available experimental methods, plus methods of analysis that necessarily go with them, in the testing of both vitalistic and mechanistic hypotheses. But even if vitalistic hypotheses could not be tested by present experimental methods, or types thereof, that fact would really create no presumption against the validity of those hypotheses. The hypothesis that there was a category of substances or factors in the inorganic distinct though not entirely dissociable from material bodies was urged for a long time before it could be put to a decisive test, and when such tests became possible the existence of energy as such a category of factors was firmly established. Experi-

mental methods had first to be developed, before that hypothesis could be tested and verified.

The history of science is indeed replete with such cases. As pointed out in another connection, the vitalistic hypothesis now has somewhat the same status as had the hypothesis respecting an imponderable energy during the seventeenth and eighteenth centuries. To cite another parallel, it has the same status as had the theory of descent during the latter part of the eighteenth century and the early part of the nineteenth. It would be just as reasonable to say that those hypotheses had no scientific validity during the period when they could not be subjected to decisive tests as it is to assert, on similar grounds, that the vitalistic hypothesis has no scientific validity at the present time.

But that claim, fallacious though it be, loses its plausibility when it is considered that both vitalistic and the opposing mechanistic hypotheses *are* being subjected at the present time to the test of more or less exact empirical data. There is every reason for believing that, as our knowledge of the organism and its activities becomes exact and detailed, more decisive tests of both types of hypotheses will be possible.

Moreover, it is a misapprehension to suppose that vitalistic hypotheses do not have substantial scientific value at the present time, for there is no question but that they are stimulating an enormous amount of biological research, both experimental and theoretical. One has only to mention the work of Loeb, Child, Jennings, Ritter, Benjamin Moore, Driesch, J. S. Haldane and others to show that a vast amount of research is now prosecuted with more or less explicit reference to the questions at issue between the vitalists and the mechanists. Indeed we may say that the life sciences to-day face in a different direction from that taken by them in the latter part of the nineteenth century, and in large part because of the development since that time of the newer vitalistic theories.

It would not, we believe, be unjustifiable to go even further and say that vitalistic hypotheses as these are being worked out at the present time have a scientific value superior to that of mechanistic hypotheses—superior in the sense that they are serving, as the mechanistic hypotheses are not, to vitalize the more fundamental problems of biology, to challenge accepted solutions of these problems, and to stimulate researches of the greatest importance in relation to this group of problems. These hypotheses are having an influence analogous to that of the great hypotheses earlier cited, those challenging the special creation of species, the material conception of energy, etc. Vitalistic hypotheses have this value quite irrespective of the final verdict which will be passed upon them by the scientific world.

The absence of all this evidence as to the practical scientific value of these hypotheses would argue nothing as to their *ultimate* scientific validity. For, taken together, they would still constitute a well grounded negative conception, one defined in terms of the limits beyond which mechanistic hypotheses may not go in accounting for the organism and its activities. Science is full of such negatives, many of them as firmly established as any positive conceptions, and, so far as we can see, it will always be so. This amounts to saying that, in the final analysis, the validity of a scientific hypothesis is not determined by the more practical uses that can be made of it. However, a valid negative conception does have its practical uses in certain types of investigation, and, more particularly, it aids in defining the limits upon the explanatory possibilities of the positive conceptions to which it has reference. But, as we have insisted, the vitalistic hypothesis is not entirely a negative conception. And there are as valid grounds for the belief that it can be given a detailed positive content as there were for the belief that the hypothesis respecting an imponderable energy could be.

Continuing our consideration of the objection that a vitalistic hypothesis can neither guide experimental researches, nor itself be experimentally tested, and the corresponding claim that only physicochemical hypotheses measure up to these criteria, we may point out that this objection and this claim contain at least two *petitiones principii,* somewhat covered up, as we shall see, but there nevertheless. The first *petitio* is that the organism must be regarded as a physicochemical system, since nothing but physical and chemical processes can be discovered therein. This assumption, which here takes the form of a *petitio principii,* has of course been considered at length in the preceding chapters, and nothing need here be added to that discussion. The fact of the *petitio* itself, however, may be made clear by pointing out some of its practical consequences, which consequences, in turn, are explicable only through an exposure of this *petitio.*

One consequence is the tendency of the mechanists, previously considered, to overemphasize the value of perceptual data in the investigation of biological questions, and to minimize the value of logical or inferential analysis in such investigations, despite the fact that all scientific investigations, those prosecuted by mechanists included, involve such analysis, and often of a complicated type.

A second consequence of this *petitio,* and one closely related to that just discussed, is the virtual demand of the mechanists that *vitalistic* hypotheses be tested by *physicochemical* data, that is, data which are yielded *directly* by observation and experiment or by mechanistic

analyses of such data. This is equivalent to the demand that the existence of one (hypothetical) category of factors be tested by data pertaining to a different category of factors, and *without* subjecting those data to the type of analysis upon which such a test depends. Or, stated differently, it is a demand that factors which, by hypothesis, cannot be seen, touched or perceived through any of the sense organs must be so seen, touched or otherwise perceived, if we are to concede their existence. This impossible demand results from the *petitio principii* indicated, and the misconception of the functions of logical analysis that goes with it.

Both sides agree of course that the physical and chemical processes in the organic, or, stated differently, the sensory data yielded by observation and experiment in this field, furnish both the point of departure for the investigation of fundamental biological problems, and the evidence by which all hypotheses pertaining to those problems must be tested. This is because, as earlier pointed out, sensory data are more susceptible of analysis, and particularly of the coöperative analysis on which science so largely depends, than are data of other kinds. In more ultimate terms, our minds are so constituted that sensory qualities seem more constant and certain, hence more real, than do other possible elements in nature. Certainty with respect to them does not depend on difficult inferential analysis to the same extent as does certainty with respect to other elements in nature, especially when the existence of these other elements is called in question. Sensory qualities and their concretions in space constitute for this reason the *point d'appui* for all investigations of nature, whatever the hypotheses entertained with respect to nature may be.

Here the mechanists and vitalists part company, however, for the latter insist on subjecting sensory data respecting the organism to a thorough analysis, which analysis demonstrates, they believe, that the physical and chemical processes identified with those data cannot alone account for the organism and its activity. The mechanists impugn the validity of such a thoroughgoing analysis, together with the conclusions to which it leads, though their own investigational procedure, in dealing with this and other problems, is not altogether consistent with such an attitude.

A *second petitio* in the objection and claim under consideration is that only mechanistic methods yield results of substantial value for biology, and that as a practical matter, therefore, the methods actually employed, at least by the mechanists themselves, are necessarily mechanistic in character. This is a *petitio* in that it implies, if it does not explicitly assert, that the organism is a mechanistic affair and therefore

to be investigated only on the basis of mechanistic assumptions. That of course is the very question at issue. The facts of the matter are (1) that biological experiment and analysis have their *point d'appui* in physical and chemical phenomena pertaining to the organism, and insofar are concerned with mechanistic processes; (2) that both analysis and experiment go beyond these processes, viewed discretely, to the organism as a whole, and necessarily concern themselves with the ensemble of the factors operating in the organism, whatever these factors may be; and (3) that experiment and analysis themselves, the methods employed in biology, could not be called mechanistic, save by a *petitio principii,* even though these are employed in investigating mechanistic processes.

We may say, in brief, that biologists cannot be dealing with purely mechanistic processes in investigating organisms, if organisms are not mechanical systems, and they do not use mechanistic methods in investigating anything, if investigation is not itself a purely mechanical activity, although investigation may be concerned with mechanical processes. That both the mechanists and vitalists employ physical and chemical stimuli in their experiments, and investigate the physical and chemical conditions of the organism and its activity, is of course obvious; but when they deal with organic processes with a view to identifying all the factors operative therein, they are in the realm of hypothesis, and for either party to take for granted that it does or does not employ mechanistic methods exclusively, or that it is dealing or not dealing with mechanistic phenomena exclusively, is to assume as settled in its favor the very question at issue.

Having exposed these *petitiones principii* of the objection urged by the mechanists against the vitalistic hypothesis, we may dispense with further discussion of them here, as the assumptions contained in these *petitiones* are of course dealt with in our more systematic examination of mechanistic hypotheses. There will, however, be some further discussion of mechanistic criteria as to the validity of scientific hypotheses and methods when we consider possible objections to the methods employed in our own analysis.

Objection from the Implied Interaction of the Unlike

It is also objected to the doctrine that there are non-material (mental and organizatory) factors in the organic, that we can form no conception of how such factors can combine with material factors in the same causal series. We grant that this is true, but if the facts prove that material and non-material factors do combine in the same causal

series, then there is no choice but to admit that such combinations can occur, or, in other words, that facts are facts. Such difficulties are presented by all connections in nature which have not shown themselves susceptible of further analysis, but that does not affect the reality of relationships proved to subsist. The connections between vital factors, on the one hand, and matter and energy, on the other hand, are no more mysterious or difficult to conceive than are the connections between matter and energy themselves. Energy, although an inorganic substance, is not, strictly speaking, a material substance, for it is not *as energy* seen, heard or perceived through any of the sense organs. Its existence is inferred from the behavior of material substances, and, through similar processes of inference, it has been shown to possess certain specific properties.

Energy indeed presents a rather close analogy to vital factors in several respects. Both are immaterial factors, being imperceptible to the senses; both are generally if not always associated with material substances (so far as we know); and both are inferred from the behavior (motions) of material substances. Naturally it has been more difficult to establish the existence of vital factors from a study of the motions of bodies, because energy was associated with the bodies suspected of having vital factors also associated; and so many of the motions of such bodies could be accounted for by energy in its known forms, that the most difficult analyses have been necessary to establish the existence of vital factors also. This was all the more difficult in that the *quantity* of the motions of all kinds exhibited by living bodies was proved to be due to energy alone. The existence of vital factors had to be proved, if at all, therefore, by showing that the *order or organization* of the motions exhibited by the living body could not be explained in terms of energy alone.

It might be of interest to point out, also, that the early history of the concept of energy presents some striking parallels with present attempts to distinguish between mental and organizatory factors, on the one hand, and material substances with which such factors are associated, on the other hand. During the seventeenth and eighteenth centuries, matter and energy were always being confounded together; and "everything intuitively believed to be real ran the risk of being regarded as material." [1] Even Boyle thought of heat as ponderable.[2] But yet the two categories were ultimately distinguished, and now lie at the very foundation of the physical sciences.

It is significant perhaps that, once the existence of energies distinct

[1] Soddy, F., *Matter and Energy,* p. 28.
[2] *Ibid.,* p. 28.

from material substances was demonstrated, physical science adjusted itself to the fact, and began to deal with energy by the same general type of experimental method that had been employed in dealing with matter, and with unanalyzed combinations of the two. But energy seems to be as much unlike material bodies, as is intelligence or organizatory factors. The existence of them all must needs be demonstrated from the way perceptible bodies behave. And if energy cannot fully account for this behavior, some other category or categories of factors must of necessity be assumed. When the existence of such factors shall have been generally conceded, we may expect science to adjust itself to the fact, and proceed to deal with them in much the same way as it has with energy.

The objection under consideration rests on the old axiom that unlike entities or substances cannot interact. But, as Boudin says, "science has already abandoned the axiom that only like can act upon like," and "is busy remaking its mechanical models in order to meet the complexity of its world." [1] Indeed, every type of interaction in nature appears to violate this axiom. Bodies and energies themselves abound in qualitative distinctions, and enter into combinations embracing elements qualitatively the most diverse. Even the simple cohesions of homogeneous chemical substances do not present an exception, for these involve an intricate interplay of masses and their motions embracing two or more sorts of factors which are quite unlike. The interaction of the unlike appears, in short, to be a universal trait of reality, and to characterize all the processes in nature of which we have any knowledge.

Alleged Incompatibility with the Laws of Motion and Energy

It will be objected, also, that the hypothesis that vital (organizatory and mental) factors influence the arrangements of matter is inconsistent with the established laws of motion and energy. The coördination of these factors with the laws of motion and energy does not fall within the province of this inquiry, as we are only concerned with the question whether or no there are factors in vital phenomena other than matter and energy. The evidence goes to show that no special type of energy is to be discovered in living organisms. But the evidence shows also that the organization of matter and energy in living bodies cannot be accounted for in terms of matter and energy alone. The evidence in the case compels us to admit, as we believe, that the laws of motion hitherto established do not adequately describe the behavior

[1] *Jour. of Phil. Psych. and Sci. Meth.*, Vol. IV, 1907, p. 534.

of living bodies. That indeed is one of the most important conclusions supported by our analysis. The evidence adduced in support of this proposition must serve as a rebuttal of the objection here under consideration.

Let us consider, in this connection, the part played in the arrangement of matter by cultural factors originating in human society. The fact is indisputable that these factors largely determine the order or arrangement of material bodies on many parts of the earth's surface, as in a modern city, for example; yet these cultural factors are not themselves possessed of any energy whatsoever. Cultural factors function in part by guiding the expenditure of energy in the arrangement of matter, *via* a certain class of organisms (human beings), that is, according to designs supplied by these cultural elements, and acted on, of course, by the organisms concerned. In other words, their function is organizatory throughout, so far as the arrangement of matter is concerned. Neither matter nor energy is created: these are organized by the cultural factors operating in conjunction with living organisms, but organized through the utilization of this same matter and energy. Matter and energy are, so to speak, compelled to supply the instruments and the power for their own organization. It is an organizing factor superimposed on matter and energy from the outside, if we may so put it.

The action of mental and organizatory factors seems to be quite analogous to the action of cultural factors. In any case, it is clear that cultural factors do coöperate in giving to a large part of matter on the earth's surface its specific arrangement, through the utilization of energy by organisms acting in accordance with designs supplied by these cultural factors. That fact having been established for one class of non-material factors (the cultural), we seem justified in inferring analogous functions for classes of non-material factors (the mental and organizatory) which are quite closely related to the former, especially since it is just the organizatory capacities exhibited by the organism that matter and energy do not account for, and that logically compel us to conclude that factors of a different sort are involved therein.

In taking our leave of this objection, attention may be called to the probability that the second law of thermodynamics does not apply in the same way to organic as to inorganic events.[1] Organisms ap-

[1] This topic has been much discussed in recent biological literature, especially by writers holding vitalistic views. A good résumé of this discussion will be found in Johnstone's *The Philosophy of Biology*, Chap. II.

parently arrest, to some extent, the universal process of energy-degradation expressed in the second law of thermodynamics. This probability lends further support to our general thesis that the activity of the living organism does not conform to the laws of motion describing the behavior of inorganic substances.

Alleged Violation of the Causal Principle

It has also been objected to the vitalistic hypothesis that it repudiates the principle of causality. This objection so obviously rests on a misapprehension of the so-called principle of causality or of the vitalistic hypothesis itself, that this misunderstanding needs only to be pointed out for the objection to be effectually disposed of. So far as we know, no vitalist has challenged the principle of causality, though at least one idealistic philosopher who inclines toward vitalistic views has done so.[1] What vitalists do challenge, of course, is the dogma that only physical causes are operative in nature. They assume, in addition, the existence of vital factors, and these are of course brought under the category of natural causes. The objection under consideration implies that only physical and chemical factors exist in nature; but this, obviously, is to beg the question at issue. The vitalists are certainly at liberty to claim that vital factors are *vera causae,* and, once they have done so, their hypothesis must be refuted or confirmed just as all other propositions respecting causes must be, that is, by an investigation of the facts bearing on the hypothesis.

The vitalistic hypothesis is by no means vague in this respect, as is often charged. For it implies that all the distinctive properties of living organisms are expressions of vital causes or factors, and these are certainly specific enough. That the category of vital factors is still largely defined in negative or relative terms can be accounted for both by the nature of the factors themselves and by the historical position of the vitalistic hypothesis at the present time. The concept of energy travelled the same road, and energy is now defined in terms that are of similar negative or relative character. These terms are now fairly exact, of course, but there are no grounds for the supposition that vital factors or causes may not also be eventually defined in specific, if not quantitative, terms.[2]

[1] Haldane, R. B., *The Pathway to Reality, passim.*

[2] Cf., regarding the objection based on considerations of the causal principle, Lovejoy, A. O., "The Iimport of Vitalism," *Science*, N. S., Vol. XXXIV, 1911, pp. 75–80. For an excellent discussion of the same objection in relation to psychophysical interactionism, see Pratt, J. B., *Matter and Spirit,* pp. 135 ff.

Applicability of Vitalistic Conceptions to Inorganic Phenomena

An objection of a different sort is that the vitalistic type of theory, if valid, would apply also to the organization of electrons into atoms, and of atoms into molecules. This objection is in the nature of an *argumentum ad hominem*, and as such could be passed by without mention. Since, however, it does, independently of that fact, serve to emphasize problems of very great importance for science, a brief consideration of it will be in order.

The contention involved in this objection appears reasonable, but its possible truth hardly creates any presumption against the vitalistic conception. It would be sounder science to hold that the contrary is true. It seems impossible to deduce the structure of the atom from the properties of negative and positive electrons or other ultimate elements organized therein. The difficulty of accounting for molecular structures in terms of the atoms combined therein does not seem, in the light of recent researches, so great. But no one can now assure us that this will be done. The working out of the vitalistic conception of life will serve to emphasize these difficulties, but also to suggest a line of attack upon them. If there is at present no way of deducing the structures of atoms and molecules from the properties of electrons and atoms, respectively, it would not seem amiss to consider the possibility that organizing factors *sui generis* are involved in these types of phenomena, and, so far as may be feasible, to formulate hypotheses and devise methods for testing this possibility. For these reasons we may say that the validity of the vitalistic conception is not impugned by the implications just considered, but that, on the contrary, these implications serve to emphasize difficulties and possibilities in other fields.[1]

Implied Intelligence of Vital Factors

Another objection to vitalism is that it assumes the existence of factors in the organism endowed with intelligence of a very high order, but factors which are nevertheless unconscious in their actions, at least in the ordinary sense of the term.[2] These factors or agents must (according to vitalistic conceptions) possess some sort of knowledge

[1] J. S. Haldane writes: "We now see physicists and chemists groping after biological ideas. No one can yet tell what conceptions will emerge from the ruins of the atomic theory; but it is at least evident that the extension of biological conceptions to the whole of Nature may be much nearer than seemed conceivable even a few years ago." *Mechanism, Life and Personality,* p. 101.

[2] Haldane, J. S., *op. cit.*, pp. 28–29.

of the physicochemical processes that occur in the organism, understand something of their import for the organism, and be able to direct those processes in the ways requisite to the maintenance of the organism.

Of all the objections urged against vitalism this seems to us the most serious. Indeed it can readily be played up in such a way as to constitute a *reductio ad absurdum* of the whole vitalistic position. It does appear improbable in the highest degree that vital factors or agents in the organism should collectively possess a greater knowledge of chemical and physical processes in the body, and greater skill in directing those processes, than, say, all the physicists and chemists living at the present time, with centuries of research back of them.

However, the case is not necessarily so desperate as that. One very large assumption made by those who urge this objection, and by some vitalists as well, is that vital agents in the organism, if there be such, are quite unconscious. A companion assumption is that regulative behavior such as physiological processes exhibit must, on vitalistic assumptions, involve intelligence of the kind ordinarily associated with consciousness.

Now, the first assumption can neither be verified nor refuted, with absolute finality, and for the reason that consciousness cannot be conclusively demonstrated by objective evidence, the only evidence available respecting the existence or non-existence of consciousness in other beings than the individual subject considering this question. This applies to members of our own and of other animal species as well as to component units of organisms to which consciousness might be imputed. But we feel justified in *inferring* from the objective evidence that other members of our species and many if not all other species are endowed with consciousness.

Is there not the same sort of justification for inferring that component units of the organism, including the cell and its parts, are endowed with something like consciousness? In considering this question it will be well to bear in mind that striking differences in size, structure and specific types of behavior between ourselves and other organisms or parts of organisms are apt to affect and even determine our opinions on questions of this nature. Guarding ourselves against prejudices from this source, let us canvass very briefly some of the more significant evidence which bears on the question.

A suggestive approach to the problem is supplied by a consideration of mental traits and capacities in the lower organisms. This group of organisms differ greatly from ourselves in the respects above specified, and if they can be shown to have much the same mental

characteristics as ourselves, though in a simplified form, we shall perhaps be more ready to consider the possibility that the component units of higher organisms are similiarly endowed. Fortunately the behavior of representative species in this group has been thoroughly investigated by Jennings, and the results of his investigations justify some pretty confident speculations as to the mental characteristics of this group.

Jennings himself has shown [1] that, so far as objective evidence can return an answer to the question (the only evidence, be it remembered, that we have to go on), we are justified in believing that the lower organisms—Amœba, Paramecium, Hydra, Stentor, etc.,—are capable of perception, discrimination, choice, attention, desire, emotion, memory, habit-formation, reaction to representative stimuli, and other functions ordinarily subsumed under the concepts of consciousness and intelligence.[2] Jennings affirms that the behavior of the lower organisms is *objectively* similar to human behavior that is accompanied by consciousness; [3] that "it is difficult if not impossible to draw a line separating the regulatory behavior of lower organisms from the so-called intelligent behavior of higher ones"; [4] and that "objective investigation is as favorable to the view of the general distribution of consciousness throughout animals as it could well be." [5] Jennings emphasizes these conclusions by the statement "that if Amœba were a large animal, so as to come within the everyday experience of human beings, its behavior would at once call forth the attribution to it of states of pleasure and pain, of hunger, desire, and the like, on precisely the same basis as we attribute these things to the dog." He goes on to say that, "in conducting objective investigations we train ourselves to suppress this impression, but thorough investigation tends to restore it stronger than at first." [6]

Does the evidence warrant a similar interpretation for the constituent units of complex organisms? We may say, in reply to this question, that it is *a priori* improbable that in the development of multicellularity in animals, *all* the mental functions which could be ascribed, on Jennings' analysis, to unicellular organisms would be lost to any of the component cells or complex groupings thereof in multicellular or-

[1] *Behavior of the Lower Organisms,* Chap. XX; Columbia University Press, publishers.

[2] Jennings does not suggest that each of the lower organisms studied by him is possessed of *all* the functions specified.

[3] *Ibid.,* p. 337.

[4] *Ibid.,* p. 335.

[5] *Ibid.,* p. 337.

[6] *Ibid.,* p. 336.

ganisms. This deduction is supported by all that we know concerning the activities of cells and other component units in multicellular organisms, for these activities are characterized throughout by the same regulative features as are the activities of unicellular organisms. Jennings himself believes that, in this matter, the same general interpretation holds for the internal processes of the organism as for its behavior as a whole. "Behavior," he says, "is merely a collective name for the most obvious and most easily studied of the processes of the organism, and it is clear that these processes are closely connected with, and are indeed outgrowths from, the more recondite internal processes. There is no reason for supposing them to follow laws different from those of the other life processes, or for holding that regulation in behavior is of a different character from that found elsewhere." [1]

Jennings thereupon proceeds to interpret these internal processes on the principles which he had worked out for the behavior of the organism as a whole. These principles are summarized as "(1) the selection through varied movements of conditions not interfering with the physiological processes of the organism ('trial and error'); (2) the fixation of the adaptive movements through the law of the readier resolution of physiological states after repetition." [2] Jennings then shows that adaptive changes in the activities of digestive glands following change of diet (Pawlow), the development of specialization in the activities of digestive organs, the phenomena of regeneration and regulative processes generally can readily be interpreted on these principles.[3]

Jennings concludes this part of his discussion with a passage very significant for the problem we are considering. "It may be noted," he says, "that regulation in the manner we have set forth is what in behavior is commonly called intelligence. If the same method of regulation is found in other fields, then there is no reason for refusing to compare the action there to intelligence. Comparison of the regulatory processes that are shown in internal physiological changes and in regeneration, to intelligence seems to be looked upon sometimes as unscientific and heretical. Yet intelligence is a name applied to processes that actually exist in the regulation of movement, and there is no *a priori* reason why similar processes should not occur in regulation in other fields. . . . In a purely objective consideration there seems no reason to suppose that regulation in behavior (intelligence)

[1] *Ibid.*, p. 339.
[2] *Ibid.*, p. 345.
[3] *Ibid.*, pp. 347–348.

is of a fundamentally different character from regulation elsewhere." [1]

It should be added that Jennings interprets regulative behavior of all types in accordance with mechanistic assumptions. This interpretation need not here concern us, as we have already dealt at length with mechanistic conceptions of life, including the organismic type of hypothesis accepted by Jennings himself. We are following Jennings only in his subsumption of regulative processes in the organism under the same general category as the behavior of the organism as a whole, and in the view that, when objectively regarded, there are much the same grounds for correlating either type of regulation with intelligence and consciousness. The question can never be decided with absolute certainty, as Jennings fully recognizes, and it may be well for that reason to assign internal organizatory factors and what we have termed sub-mental factors to separate categories. This, of course, we have done. The general hypothesis that internal processes are characterized by a type of regulation comparable with intelligent behavior has been justified independently of that distinction. By showing that those processes can be so interpreted, the objection here under consideration is effectually rebutted. Some further elaborations of the topic will, however, be in order.

One assumption implied in this objection, we pointed out, is that intelligence of the type presumably imputed by the vitalists to organizatory factors must be accompanied by consciousness. Granting for the moment that vitalists must endow these factors with intelligence, it does not follow that such intelligence is accompanied by consciousness in the ordinary sense of the term, and particularly by awareness or self-consciousness. Regulative processes in the organism may be more nearly analogous to behavior guided by instinct and habit than to behavior guided by reflective thought. Jennings' analysis of the matter would suggest that this is the case. If that be so, then the rôle played by vital factors in internal regulative processes need not involve intelligence of a very high order, or little if any consciousness or awareness. But of course we are unable to say just what the facts of the case are. There may be both intelligence and consciousness, or there may not be. There is, however, something analogous to intelligence, though this something need not rank exceptionally high, compared with human intelligence.

Calling this something intelligence, for lack of a better term, let us consider an objection which may be urged against the view just stated. This objection is that the vitalistic interpretation of regulative processes must necessarily impute intelligence of a high order to the vital

[1] *Ibid.*, p. 349.

factors hypothetically involved, considering the highly adaptive character of those processes, and the superior knowledge and skill which it implies.

This objection loses much or all of its force, once attention is directed to the long period of time during which this regulative capacity and this knowledge, if we can call them such, were acquired. Present-day organisms are all heirs, in some sense and degree, to the knowledge and the capacities acquired by their ancestors for thousands and thousands of generations. The knowledge and regulative capacities implied in the behavior and the internal processes of organisms now living were undoubtedly developed, bit by bit, during all that period. This knowledge and these capacities do not, consequently, imply an intelligence of a very high order, any more than do, on a different level, the complex institutions of many human societies, or the complex societies of bees and ants. All these complex and adaptive arrangements have developed very gradually, by a process of cumulative growth. As in human societies and, to a lesser extent, among the social Hymenoptera, the complex organism may and apparently does represent a great ensemble of specialists, each with very useful and very precise knowledge, but knowledge of very limited scope, yet with enough general knowledge, and enough intelligence and coöperativeness as well, to assure the maintenance and perpetuation of their highly complex society. That this knowledge, intelligence and coöperativeness are nevertheless very limited, the extinction and arrested development of species, the maladaptations of all species to their environment, the ills to which they are all subject and the high mortality rates which all of them suffer amply demonstrate. The analogy to human societies is obvious.

Some qualifications of the foregoing analysis would of course be necessary before it would be applicable to plant species. But the regulative processes in the latter are fundamentally similar to the corresponding processes in animal organisms, and the same general interpretation would apply to them. Recent researches have served to emphasize these fundamental similarities between the two kingdoms. We may, therefore, dispense with any detailed qualifications of the analysis with a view to its application to plant species.

We may say on the basis of these considerations, speculative though they be, that none of the implications of a carefully formulated vitalistic conception of the organism are in conflict with the objective evidence respecting the endowment of the component units of the organism with functions of a sort sometimes accompanied

by consciousness and intelligence. We are obliged, on the contrary, in view of the insuperable difficulties encountered by all mechanistic conceptions of life, to accept some sort of vitalistic conception, together with certain of its implications respecting the rôle of intelligence or something comparable therewith in the regulative processes of the organism.

Implications of Bastian's Work

We wish now to anticipate and consider an objection to the conclusions yielded by our inquiry, which, as we shall see, may prove to be a very serious one. We refer to implications which would undoubtedly be drawn from the not unlikely demonstration that living organisms may now arise from inorganic sources, and in particular, from certain types of saline solutions.

Bastian has long been testing this possibility,[1] and claims to have succeeded in obtaining considerable numbers of micro-organisms, including micrococci, torulae and moulds, from certain saline solutions hermetically sealed, superheated, and then exposed for various periods of time to diffuse daylight. Two different solutions in distilled water were used, both containing sodium silicate, and one of them containing, in addition, ammonium phosphate and dilute phosphoric acid, the other per-nitrate of iron. The solutions were heated to from 125° to 145° C., temperatures much higher than the thermal death-point of the organisms obtained. The period of exposure to diffuse daylight, in the experiments yielding positive results, ranged from three to twelve months. Colloids were formed in these solutions, which was considered the most significant feature of the experiments on the physicochemical side. The evidence that real organisms were obtained in these experiments is found in their microscopic appearance, staining properties in relation to certain reagents, and reproduction under proper nutrient conditions.

So far as a layman can judge, these experiments, especially the later series thereof, were properly controlled, and we seem justified in provisionally accepting their results. The experiments should of course be repeated by other workers, but at the time Bastian's book was published (1911) this had not been done. The attitude of the scientific world toward these experiments has been one of indifference or open

[1] Bastian, H. Charlton, *The Origin of Life*. This fascinating little book contains an account of the author's experiments on this problem, with a detailed description of the methods employed and the results attained, including plates showing some of the organisms said to be obtained in these experiments. There is a sympathetic account of these experiments in Moore's *The Origin and Nature of Life*, pp. 193 ff.

ridicule, owing undoubtedly to the fact that the impossibility of "spontaneous generation," believed to have been demonstrated once for all by Pasteur and others, was erected into a sort of dogma which virtually outlawed further research on the subject. But there are indications of an open mind on the part of some scientists with respect to the question and we may expect it to be thoroughly investigated, eventually, on the basis of Bastian's methods.

What effect will Bastian's results have on vitalistic conceptions of life, in the event of their confirmation by other experimenters? Bastian himself offers a physicochemical interpretation based on conceptions of colloidal behavior,[1] and other physicochemicalists would of course accept similar interpretations. Would our analysis of physicochemical hypotheses apply to such interpretations of these particular results, in the event of their confirmation?

We can find no grounds for the belief that it would not, granting its validity with reference to physicochemical conceptions of vital phenomena generally. We have already pointed out, in discussing Lotze's mechanico-teleological conception of the organism, that vital factors of the sort implied by our analysis would be as competent to initiate the organization of matter into living forms as to maintain its organization in those forms. The confirmation of Bastian's results would *seem* serious for vitalistic conceptions of life, because the origin of life would apparently be deprived of its obscurity and much of its mystery; and the all but invincible eye-mindedness of most people, including most scientists, would at once favor the claim that these experiments had completely vindicated the materialistic conception of life, save in the case of those who had critically examined the implications of that conception. But in reality no additional grounds would be supplied by these results for the physicochemical interpretation of life. For it could scarcely be maintained that the walls of hermetically sealed vessels offer any greater obstacle to the invasion of vital factors than did the environing conditions of the matter which first took on the form of life, whether on the earth or some other planet.

Methods of the Analysis

We shall, in conclusion, consider objections which may and quite likely will be raised against the methods employed by us in the examination of mechanistic conceptions of life. This examination has been in the nature of a logical or inferential analysis of those conceptions, though always employed with reference to the empirical evi-

[1] *Op. cit.*, pp. 92–99.

dence in the case. Stated differently, the empirical evidence has been subjected to a more systematic analysis than is commonly undertaken in the treatment of such problems. This type of analysis, as exemplified in the present inquiry as a whole, has been defended in other connections: First, as it was employed in the examination of Lamarckian and anti-Lamarckian hypotheses respecting the origin of active adaptations; and, secondly, as it was by implication attacked in the objection to the vitalistic conception alleging it to be scientifically sterile. We need not review, in the present connection, the detailed defense, in those discussions, of the general method employed in our inquiry, but only indicate, in a summary fashion, its bearing on our examination of mechanistic hypotheses.

To begin with, mechanistic and vitalistic conceptions of life, like Lamarckian and anti-Lamarckian hypotheses respecting the origin of active adaptations, can be correctly appraised only through investigations in which systematic analysis, that is, analysis of a type usually termed logical or inferential, is especially emphasized. The same type of analysis is involved in all scientific investigations, but an extensive use must be made of it in the investigation of complex problems, such as are presented by the regulative processes of the organism. This group of problems are, however, not peculiar in that regard, as could be readily shown.

The general method in question found its specific application, in this part of our inquiry, in the examination of the mechanistic contention, and the vitalistic denial, that the behavior and the internal processes of the organism can be wholly accounted for in terms of physical and chemical changes. Complicated analyses of the type termed logical were necessary in investigating this problem, for the reason that, as we showed, the relevant data, when analyzed on the basis of the methods employed by the physicochemical sciences, do not really touch the problem, but only yield results which apparently support the mechanistic side of the controversy. This is because the methods themselves prejudge the issue, so to speak, and, in effect, beg the question in favor of the mechanists. In other words, the only type of evidence yielded by these methods is evidence respecting physical and chemical processes in the organism. Needless to say, such evidence can be discovered *ad libitum*. These methods in themselves can never take us beyond such discrete processes, and throw any light on the complex regulation or organization of those same processes, because processes of the same general type are assumed to accomplish this regulation, and the regulation of these regulative processes stand in equal need of explanation, and so on *ad infinitum*. For

this reason all the data adduced by the mechanists in support of their hypotheses necessarily incorporate the very question at issue, and serve, in the main, only to illustrate it.

We therefore had to devise different methods of dealing with the problem, and through the use of these methods subject the assumptions underlying the mechanistic conceptions to a critical analysis. This, needless to say, was done over the putative protest of the mechanists. But it could be shown that the type of analysis employed is necessarily involved in the investigation of all scientific problems, including investigations by the mechanists themselves.

It might perhaps be said in defense of the mechanists that their attitude in this matter is largely due to the circumstances of their training and experience. They have become so inured to the methods and assumptions of the physicochemical sciences, together with the peculiar types of logical analysis involved therein, that methods, assumptions and types of analysis to which they have not thus been habituated are uncritically put down as spurious. In any case their protest against the types of analysis employed in the present inquiry could scarcely be deemed a justifiable one. Methods of investigation must be adapted to the specific problems investigated, and the problems with which we have dealt cannot be resolved by the methods of physics and chemistry, but must be treated by methods of the general type exemplified in our inquiry. Physical and chemical data bear on, and might indeed be said to create, these problems, but these data must be appraised in relation to these problems, through the use of methods other than those employed in the sciences yielding such data. Our analysis has had the same scrupulous reference to these data as any so-called mechanistic analysis could have and, if the foregoing considerations be sound, has made a more legitimate use of these data than could any mechanistic analysis thereof.[1]

[1] It may be well to quote in this connection some words of an eminent physiologist on the necessity of philosophical analysis in scientific work. "Apart from philosophy, which looks at experience as a whole, we can never reach clearness about the knowledge dealt with in individual sciences; and this is why so much philosophical discussion has entered into these lectures. Just as an ordinary workman ought to understand his tools, their dangers, and what can or cannot be done with them, so ought a man of science to understand those tremendously powerful and dangerous logical tools with which he does the work allotted to him. Nothing is more wide of the mark than the contention that philosophy is not needed in scientific work. The inevitable result for men of science who ignore the history and results of philosophy is that they are far more apt to fall victims to all sorts of misunderstanding, and even to gross superstition. They are also apt not to see great scientific questions which are waiting for experimental investigation; and they are in danger of spending time fruitlessly on investigation which can lead nowhere." Haldane, J. S., *Mechanism, Life and Personality,* p. 151.

Summary of Results Yielded by the Analysis of Mechanistic Conceptions of Life

We may now summarize the conclusions supported by the analysis presented in this and preceding chapters, of mechanistic conceptions of life.

We presented a comparison of mechanistic and rival hypotheses as to their logical characteristics, and concluded that neither group of hypotheses occupies a logically privileged position compared with the competing group.

We also compared animal organisms with inorganic substances in regard to the sensory elements entering into their respective compositions; and showed, by means of this comparison, that animal organisms embrace sensory elements of a sort not entering into the composition of inorganic substances. Since the term sensory elements denotes an epistemologically neutral conception of matter and energy, this demonstration is tantamount to a proof (1) that current conceptions of matter and energy are incomplete, in that they do not allow for sensory elements peculiar to animal organisms, as indicated by the organic and motor sensations of the latter; and (2) that animal organisms cannot therefore be accounted for, as to their physical components, on the basis of current conceptions of matter and energy. It was also pointed out, in this connection, that the stimuli of organic and motor sensations, and hence essential components of the elements indicated thereby, are identified with *specific organizations* of physicochemical processes in the organism. (A later part of the analysis purported to show that organization of this type is not explicable in terms of the associated physicochemical processes alone.)

It was further shown, by a deduction from this series of conclusions, that mental states constituted in part by organic and motor sensations cannot be accounted for in terms of physicochemical processes as currently conceived, because not allowing for physical elements indicated or constituted by those groups of sensations. It was argued, also, as against certain radical empiricists, that cognitive processes, affectional states as wholes, and other complex entities usually termed mental are peculiar to living organisms alone, and constitute additional differentia between organisms and inorganic substances.

We then undertook to show that the part played in animal, particularly human, behavior by these mental factors, and more especially by cognitive processes, could not be accounted for in terms of matter and energy, because such behavior is directed toward objects or events which are remote in space and time from the organism,

and with which the organism is not effectively related, in an energetic sense. It was pointed out that this line of evidence refutes (1) all physicochemical conceptions of the organism, with their various epiphenomenalistic, parallelistic and two-aspect doctrines of mind-body relationships; (2) the doctrine of radical empiricism that minds and physical objects are composed of the same sorts of neutral entities; and (3) relational theories of consciousness affirming or implying that mental processes are constituted by relations among *physical* objects.

We then undertook a factorial analysis of human experience, particularly of complexes therein termed physical and mental. The main types of epistemological theory with regard to this general problem were critically examined, including neo-realism (in two of its forms), positivism, radical empiricism, subjectivism, objective idealism, absolutism and critical realism. The positive conceptions supported by this analysis were deemed to approximate the basic positions of critical realism, when regarded from the epistemological point of view.

A doctrine as to the categories of factors entering into human experience was formulated on the basis of this analysis. The doctrine holds that sub-mental and sub-physical entities are essential factors in all our experience, these terms standing for entities approached in various degrees by what, according to common usage, are regarded as mental and physical processes, respectively, but which are not given in experience in a pure form, being synthetized together therein in indefinitely diverse ways. It was further concluded, on the basis of this analysis, that other distinct categories of entities enter into human experience, namely, organizatory factors, cultural factors, space and time, and logical and mathematical entities.

This general doctrine was then tested and elaborated by analyzing various types of complex entities in terms of entities subsumed under the fundamental categories specified. We represented this doctrine to be of a provisional character only; and disclaimed any special responsibility for the elaboration in connection therewith, of ontological problems pertaining to space and time, and to logical and mathematical entities. The categories of organizatory and cultural factors were reserved for further elaboration in subsequent chapters. We recognized, of course, that this part of our discussion transcended the limits represented by a critical analysis of mechanistic conceptions of life, though dealing with problems necessarily touched on by such an analysis.

The physiological processes of the organism and the associated physicochemical processes were then subjected to a comparative analy-

sis. It was concluded from this analysis that processes of the two types are incommensurable. It was shown in particular that the fairly constant and uniform relationships between the discrete physicochemical processes of the organism are not accounted for by these processes themselves; or, in other words, that these relationships are not physical or chemical in character. These relationships were defined as qualitative, quantitative, temporal, spatial and, in certain phases of the organism's functional activity, as logical in character. Relationships of the first four types were subsumed under the general concept of organization, and the corresponding terms were designated as organizatory factors or agents; logical relations, on the other hand, were deemed to implicate (when involved in animal behavior) mental (sub-mental) factors, being dependent thereon for their discovery and practical application.

The processes of variation and inheritance as interpreted on physicochemical conceptions of the organism, and on mechanistic conceptions not explicitly formulated in physicochemical terms, were then subjected to an analysis of the same type; and this analysis resulted in similar conclusions, namely, that the qualitative, quantitative, spatial and temporal relationships subsisting between the physicochemical processes involved are not interpretable in terms of the latter, but represent organization of a non-physical type and point to the operation, in this group of phenomena, of organizatory factors. It was shown that this analysis carries critical implications for current doctrines assuming the independent variability of factors in the germ-plasm, and of the corresponding somatic characters (mutations); and for Lamarckian, anti-Lamarckian and other theories as to the factors in hereditary variations, when these are construed in a mechanistic sense.

We then tested physicochemical conceptions of life by an altogether different method, that of concomitant variations, and showed that nowhere is there a perfect correlation between the physiological or mental processes of the organism, on the one hand, and the associated physicochemical processes, on the other. It was shown, in particular, that correlations of comparatively low coefficients subsist (1) between the morphological and functional characteristics of the various taxonomic groups, and the types of chemical processes associated therewith; (2) between the individual organism, and the numerically identical chemical bodies combined therein; (3) between numerically identical memories, habits and other mental functions, on the one hand, and the numerically identical physicochemical processes associated therewith, on the other; (4) between various *types* of mental functions, and

the types of the associated physicochemical processes; (5) between thought and other cultural factors originating in human society, and brain and other material processes involved in the origination, transmission and operation of those factors. We concluded that the correlations thus determined are of such an order as to create a strong presumption against the validity of physicochemical conceptions of life, and in at least certain directions definitely to overthrow those conceptions. A corollary of this conclusion is that a class or classes of factors are operative in the organic which are quite distinct from the physicochemical factors also operative therein.

We pointed out from time to time that the conclusions yielded by the different methods employed in the analysis are quite consistent with one another and indeed, so far as comparable, substantially identical. It was suggested that this fact supplies specially weighty testimony as to the validity of the conclusions attained through the employment of these several methods.

Probable objections to the methods of the analysis and to the positive conceptions supported thereby were then considered at some length. This discussion, set forth in the present chapter, need not be recapitulated. It has shown, we believe, that none of these several criticisms successfully impugn the legitimacy of the methods employed in the analysis, or seriously compromise the positive conceptions yielded by the analysis. We recognize, of course, that errors are inevitable in any systematic treatment of problems so difficult; the critics, however, will doubtless be more than willing to expose such errors in our own treatment.

CHAPTER XIII

A SYNTHESIS AND QUALIFICATION OF RESULTS YIELDED BY THE INQUIRY

THE conception of life implied in our critical analysis of various hypotheses dealing with this question has been indicated only in a fragmentary fashion. The primary object of that analysis has been to appraise critically current conceptions of vital phenomena which seemed inadequate, and not to elaborate systematically an alternative conception synthetizing and extending the positive contributions of those conceptions, while avoiding the fallacies from which they were shown to suffer. Nevertheless more positive views were expressed here and there, and these may now be assembled, elaborated into a systematic theory of life, and employed as a test of current vitalistic conceptions of life phenomena, the only remaining group of hypotheses on this subject still to be examined.

Our examination of physicochemical conceptions of life led to the conclusion that the organization of physical and chemical processes peculiar to living organisms cannot be accounted for in terms of the physical and chemical processes themselves. We were obliged to infer, consequently, that a distinct category or categories of factors are operative in organismic activities, and that to these must be attributed the organizatory functions exhibited in all such activities. Factors of this group were defined in both negative and positive terms. By the negative definition, they subserve organismic functions which cannot on a rigorous analysis be imputed to the chemical and physical processes of the organism. By the positive definition, they subserve all the organizatory and regulative functions of the organism. They are expressed, positively, in organic structure, the processes of reproduction, heredity and development, the genesis of hereditary variations, the regeneration of lost or mutilated parts, the organization and regulation of the more common physiological processes, and the adaptive behavior of the organism as a whole.

Negative definitions of these factors have hitherto been more emphasized than have the positive definitions, but this has been due largely to the dominance of mechanistic conceptions of life, and, consequently, to the necessarily destructive character of work undertaken in behalf of opposed conceptions; also to the fact that, owing

to the constitution of the human mind, sensory qualities subsumed under physicochemical categories constitute the data, the points of departure, for the construction of all theories of nature, living organisms included. But, as we have shown, vital factors are susceptible of positive definitions, and we may expect such definitions to become more specific and detailed concomitantly with progress in the systematic analysis of vital phenomena, by which, more and more, this group of problems must be attacked.

In more explicit terms, vital factors are operative wherever the relationships among the physicochemical processes of the organism conform to more or less exact qualitative, quantitative, spatial and temporal specifications, which do not characterize the given physicochemical process as such. Stated differently, the specific interrelationships of discrete physical and chemical processes going on in the organism, insofar as such interrelationships are *characteristic* of the given organism or of the given taxonomic group, must be credited to factors of this category. Adaptive behavior of the organism as a whole involves factors of the same general category, particularly mental (or sub-mental) factors. Such behavior also often if not generally calls into play relations of a logical order, while still involving relations of the types above specified.

Certain qualifications of these conceptions may now be indicated. One is that genuine mechanisms of various sorts are developed by organizatory factors operating in conjunction with physical and chemical factors. Many physical and chemical processes in the body are controlled by these mechanisms. This qualification, however, in no wise invalidates our previous analysis of mechanistic conceptions. For, first, those mechanisms have been *built up* under the direction of organizatory factors, as a systematic analysis of morphogenesis would show; secondly, they are *maintained* through the agency of factors coming within the same category, as would be shown by a systematic analysis of the metabolic processes involved in their maintenance; and, thirdly, these specific mechanisms do not, when taken together, constitute a genuine machine, as has been demonstrated, but are organized on different principles, and through different factors, than the mechanistic conception would imply.

Another qualification is that the specifications of qualitative, quantitative, spatial and temporal sorts to which physical and chemical processes in the body conform are not absolutely rigid. On the contrary, deviations from and even downright violations of many of those specifications may be tolerated. Pathological conditions, mutilations and injuries which are not fatal connote such deviations from or vio-

lations of the specifications to which the physicochemical processes of the organism normally conform. And of course the specifications in question are not absolutely the same for different organisms or for the same organism at different times. It is unnecessary for present purposes to set forth in detail the sorts of deviations from these specifications which may be allowed, or, in other words, the limits within which the specifications may be varied. But these qualifications are really allowed for in the general conceptions we have proposed.

We may now effect a closer synthesis between the functional activity of the organism and the vital factors therein, as just defined. Physiological processes in the organism conform, of course, to specifications of the types previously characterized. They are therefore under the direction of vital factors at work in the organism. Stated more precisely, any specific functional activity comprehending two or more discrete physicochemical processes interrelated according to specifications of this sort are controlled by vital factors operating in conformity with the given specifications. Functional activity is of course embodied in the chemical substances of the organism, and embraces the physicochemical behavior of those substances, but it comprehends a type of processes additional to these physicochemical components.

Relating these deductions to the problem of active adaptations, we may say that the part played by functional activity in the genesis of such adaptations must be credited in large part to the vital factors involved therein. These, as we recognized, are always conditioned and often seriously limited by the physicochemical conditions with which they must work, but the net adaptive achievements, if we may put it thus, are not the result of those conditions. Active adaptations are vital in character, and must, considered in this aspect, be credited to the category of factors designated by that term.

We have presented in the genesis of active adaptations both a positive attribute of vital factors, and a limitation, often of a very serious character, on their operation. Vital factors not only organize functional activities, but the later organizations of these activities are a development from the earlier organizations thereof. This general principle applies both to the evolution of species or other taxononic groups, and to the development of individual organisms. Vital factors are therefore modified through their own activities, or the activities which they organize. In anthropomorphic terms, vital factors seem to work everywhere on the basis of experience. This, however, is not necessarily a metaphorical way of putting the matter,

as the part played by intelligence, or something comparable therewith, in the internal processes of the organism, as well as in its behavior as a whole, would testify.

We might by bringing together specific analyses undertaken at various points in our inquiry justify this general proposition that all the truly organizatory work of vital factors is based on past experience or functional activity. The intelligence, or something analogous to it, which could, on the objective evidence, be imputed to the internal processes in the organism; the time required for adaptive modifications of physiological processes to work themselves out, after a change of conditions affecting them (as in the activities of digestive glands after a change of diet); the necessity which we recognized for ascribing some influence to functional activity in the genesis of all types of active adaptations; the fact that active adaptation of component units within the organism to one another, and of the organism as a whole to its environment, is of the very essence of vital activity—all this, with facts and considerations of a similar nature, would seem to warrant the conclusion that vital factors are always guided in their work by past experience or activity. This proposition could be further supported by showing that, since all types of organization or regulation in the body involve the adaptation of physicochemical processes to one another, our former analysis of active adaptations, with its vindication of the Lamarckian principle, would apply to all these types of regulation or organization. If this be true, the vitalistic and the Lamarckian principles, as thus synthetized, are exhibited in all vital phenomena whatsoever, for all vital processes conform to them. Physicochemical processes are everywhere present, too, of course, conditioning and limiting the operation of the vitalistic and Lamarkian principles, but nevertheless organized in conformity therewith.

The limitation on vital factors, implied in their guidance *by* functional activities, is evidenced, empirically, by the relatively conservative character of vital organization, by its imperfect adaptation to environment, as a consequence, and, on the other hand, by an excessive development of hereditary characters which often unfits the organism for survival. The tenacity of vestigial structures, persistence of more or less archaic mental traits and mal-proportioned development of specific hereditary characters are widely distributed phenomena in nature, and indicate the extent and seriousness of the limitations upon vital factors represented by the condition in question. Analogous phenomena in individual development will suggest themselves.

Evidences of other limitations might be added. The extinction,

degeneration or arrested development of many forms of life, the exceedingly slow pace marked by the evolution of other forms, the relatively slow rate of evolution along every line, the dependence of all species upon quite specific environmental conditions, such as certain kinds of food, a limited range of temperature or a particular medium for oxygenation and locomotion, the unequal or unequivalent distribution of organizatory and mental capacities themselves, as in animal and plant kingdoms, or in the various phyletic groups within these kingdoms, and, in short, all those diverse combinations of vital properties presented by living nature represent limitations on the vital factors operative in life, and the types of organismic activities organized thereby. To these we should have to add limitations represented by the kinds and amounts of matter and energy requisite to the maintenance of organisms of the various species, limitations which are closely related, of course, to the amount of organizable matter and energy available for vital activity in general. Specific mention should be made, finally, of the limited capacity for learning (or that which is analogous thereto) characteristic of all vital factors or agents. This is probably responsible, in large measure, for the extinction, degeneration, mal-adaptation, arrested development and slow rate of evolution exhibited by various groups of organisms.

Our sketch so far has distinguished two broad categories of factors in vital phenomena, the material, comprehending various forms of matter and energy, and the non-material, comprehending all the factors that subserve regulative or organizatory functions in the vital process. The latter category may now be subdivided into the more specific categories distinguished during the course of our analysis.

One of these more specific categories has been designated as the organizatory. To this category of factors are attributed the various organizatory and regulative processes within the organism. Stated differently, their office is to establish, maintain and, on occasion, modify the organization of physicochemical processes in the body according to specifications of the various types previously characterized, and to build up, maintain and coördinate the mechanisms which, to a large yet limited extent, control these processes. Reproduction, inheritance, ontogenesis, regeneration, metabolism, specialized physiological activities, the coördination of those activities, and regulative processes evoked by changed conditions affecting these various systems of activity constitute the field wherein this more specific category of vital factors do their work. This group of factors, as we saw, are endowed with a species of intelligence, or attributes comparable therewith.

Another specific category of factors, though only provisionally set off from the category just reviewed, was designated by the term mental, or sub-mental, the latter a new term denoting one of two correlative categories of factors operative in animal behavior and experience, sub-physical factors constituting the other category in this combination. Employing here the more familiar term, it is the category of vital factors termed mental whose special office it is to direct the responses of the organism as a whole. Behavior is the term commonly employed to designate this group of responses.

Behavior, considered from the biological standpoint, is the process whereby the organism adjusts itself to its environment. These processes of adjustment, however, are not always of a utilitarian character, in the sense that they serve to maintain the life of the organism, or to perpetuate the existence of the species to which it belongs. Many activities mediated by mental factors are apparently in the nature of superfluities or luxuries, considered from this more utilitarian standpoint. Illustrations of such activities would be found in the cultivation of art, in the speculative contemplation of nature, and perhaps in the pursuit of so-called higher interests generally. Analogous activities could doubtless be identified among the lower organisms.

These two specific categories of vital factors, the organizatory and the mental, are of course bound up together, as the activities which they severally direct are inseparable. The adjustments of the organism to its external environment affect more or less vitally its internal processes, and, conversely, the latter affect the adjustments to environment. More specific indications of the interrelationships between the two groups of factors are found in the phenomena of psycho-therapeutics, the dependence of mental functions on the brain and nervous system, the effect of digestive processes, secretions of the ductless glands and other physiological changes on mental states, and the influence of cultural factors, largely a product of mental factors, on modes of living and, through these, on the internal processes of the organism. Indications of a different sort are found in the activities of the lower organisms, in which the differentiation between mental and organizatory factors, if present at all, is not carried so far. We should perhaps be justified in assuming that the differentiation between the two categories of factors, supposing the distinction to be a legitimate one, is an outcome of the evolutionary process, and that this differentiation becomes sharper the greater the degree to which specialization of structure and function is carried.

A third specific category of vital factors comprehends knowledge, arts, customs and other cultural controls originating in the past of

the social group as such. These again are not sharply marked off from the first two categories of factors, being originated, transmitted and assimilated, as they are, by organisms in which those categories of factors are operative. They largely control the operation of mental factors in human society, as the influence of language, science, arts, institutions and other cultural factors demonstrates. Through this control they also exercise a good deal of influence over the physiological activities of the organism. Illustrations of such influence may be found in the effect on physiological activities, of dietaries, narcotics, intoxicants, medications, sex mores, types of occupation and modes of locomotion developed or discovered in the past. Cultural factors play a much more important rôle in human activities, of course, than in the activities of other species. They are indeed relatively unimportant in the case of other species, though there is good evidence that they enter into the activities of certain other species.

To complete the enumeration of factors operative in animal, and especially in human, activity, we should have to add to the various categories of material and vital factors, other factors distinct, ontologically, from any of these categories. These as treated in a previous chapter would include space, time, and logical and mathematical entities. Any detailed analysis of these factors, if we may term them such, and of their relationships with categories of factors subjected to such an analysis, falls outside the scope of our inquiry. We have been concerned only to indicate their connection, in a general way, with our special problems. It is perhaps sufficient for our purposes to say that all organisms have their place in the spatio-temporal order, whatever its character, and that logical and mathematical entities enter, in some sense, into the activities of animal, and particularly of human, organisms.

A point of cardinal importance, but one stressed so often that perhaps no added emphasis need be given it here, is that these various categories of factors, while distinguishable one from another, are by no means separable *in* vital activity, but are all bound up together. Nevertheless, factors of most if not all these categories seem capable of subsisting separately from one another, outside living organisms. Some of the grounds for this latter conclusion may be reviewed.

Chemicals and energies of the sorts combined in organisms can and do exist in inorganic forms, of course, though it does not seem possible to dissociate matter and energy themselves, except within certain limits.

There is some empirical evidence that mind may exist apart from organisms as we know them, though this evidence is not absolutely

conclusive, and many scientists will have nothing whatever to do with it.[1]

Cultural factors are in a sense capable of subsisting independently of living organisms, but not, apparently, of matter and energy. As we have fully recognized, however, they are dependent on organisms for their practical influence.

We have some evidence that organizatory factors may exist apart from material substances. Should the results of Bastian's experiments on the development of life from inorganic sources be confirmed, we should have to concede the existence of organizatory factors apart from material substances. Our general analysis carries the same implication, indeed, since the origin of life in connection with previously unorganized chemicals and energies can be accounted for on our theory only by assuming that disembodied vital factors invaded those substances and organized them into living forms. Similar considerations support the conclusion that mental or sub-mental factors must at some time have existed apart from matter and energy; whether they could also exist apart from organizatory factors is not suggested by our analysis.

According to the discussion in a previous chapter, space, time, logical entities and mathematical entities appear to be severally capable of subsisting apart from entities of other categories.

Attention should perhaps be directed to what may be called the social aspects of vital activity. The organism is in a sense solidary with its environment, and cannot be conceived apart from it. This has been sufficiently insisted upon already. It may be pointed out, however, that if our theory of vital activity be at all correct, the environment is a very complex affair, for it not only includes inorganic conditions, but also organisms of various species, in which factors of various categories herein distinguished are operative. That fact signifies, of course, that these various categories of factors interact not only *in* the organism, but also by and through the reactions of organisms to one another. And interactions of the latter type are exceedingly significant, as the struggle for existence, the processes of natural selection, the hereditary adaptations of species to one another, the social organization of numerous species, the domestications of one species by another, the coöperation of individuals in the development of culture, the part probably played by social life in the evolution of hereditary mental traits, etc., would all testify. The diverse groupings of factors thus brought into interaction are of equal significance.

[1] We refer to the evidence being accumulated and analyzed by the societies for psychical research, so-called.

The geographical distribution of plants and animals, differences and changes in physical conditions on the earth's surface, the evolution of living organisms themselves all connote such diverse combinations of factors.

Finally, some substantial qualifications of the theory just sketched would be entailed in its application to the plant kingdom, to the lower animal organisms, and to organisms standing between animal and plant kingdoms. Physicochemical processes in plants and animals are quite differentiated, of course, and there are lesser but substantial differentiations between the various taxonomic groups, and even between individuals in the same specific groups. Cultural factors are not operative in the activities of plant and the lower animal organisms; organizatory and mental factors are not clearly differentiated, if at all, in the lower animal organisms; and it is not certain, though the hypothesis has been entertained, that plant organisms are endowed with mental as distinguished from organizatory capacities. Other qualifications for these various groups will readily suggest themselves. Within the specific groups themselves the distribution and interrelationships of these various factors are infinitely diverse, for, as has been commonly assumed, no two living organisms are exactly alike, and possessed, therefore, of precisely similar vital, physicochemical or other classes of properties.

CHAPTER XIV

VITALISTIC CONCEPTIONS OF LIFE

We shall now examine, from the standpoint of the theory just sketched, various types of vitalistic theory recently elaborated, devoting the major share of our attention to the theories of Bergson and Driesch, and treating but briefly the views of other vitalistic writers.

Bergson's vitalism has been formulated with reference to the problems of phylogenesis, while Driesch, largely because of his negative attitude toward current hypotheses respecting the processes of evolution, has formulated his theory with reference to the development of the individual organism. The two theories are in substantial agreement as to essentials and, when taken together, constitute a more or less comprehensive theory of vital phenomena. Our analysis of these theories will be both critical and constructive: critical, in directing attention to features thereof which cannot be made to square with a more synthetic theory of life, such as we have proposed; and constructive, in indicating how their undeniably valuable contributions to the general problem can be synthetized in a more comprehensive theory of vital phenomena.

Vitalistic Evolutionism According to H. Bergson

We shall restrict our examination of Bergson's theory to his doctrine of the vital impetus, which corresponds roughly to the specific category of factors herein termed organizatory, though the attributes with which he clothes the vital impetus are quite different, in certain respects, from the attributes assigned by us to the corresponding category of factors.[1]

Vitalism in the form which Bergson has given it may not appear to fall under the criticism we have been directing against theories which find in certain properties of life the primary factors of organic evolu-

[1] Bergson's theory of the vital impetus is set forth in his *Creative Evolution*. In earlier works—*Time and Free Will*, and *Matter and Memory*—he has maintained and gone far toward demonstrating the incommensurability of mind and matter. These works may therefore be taken as complementary contributions to the vitalistic theory formulated in the more recent work, and, more particularly, as supporting the thesis that a distinct category of mental factors is operative in vital phenomena.

tion, since it attaches a certain importance both to "use and disuse" and to physicochemical processes associated with vital activity. A more attentive examination of Bergson's theory will show, however, that it assigns to functional and material factors quite subordinate rôles in the evolutionary process. It is assumed, moreover, that credit for specific evolutionary changes may be apportioned, at least in principle, among the several factors, or properties, of vital phenomena.

Bergson rejects both the mechanistic and finalistic interpretations of life, setting up instead his doctrine of an original "vital impetus" which, acting as a *vis a tergo,* is the fundamental cause of hereditary variations and of evolutionary processes generally. Life does not always work according to a plan, as the finalists claim, nor are its forms to be attributed, on the other hand, to the interaction of physicochemical forces: there are, so to speak, certain impulsively determined directions of evolution at the start. Thus may be explained the parallel development of sexuality in the higher plants and animals, and even the development of similar specific structures on independent lines of evolution. More general changes are susceptible of a like interpretation. Life could not realize its potentialities in a unilinear series of development. It was obliged to split up into plant and animal kingdoms, for plant and animal characters as we know them could not develop in the same species. Species themselves had to be indefinitely multiplied in number, so that the vital impetus might be scaled down, as it were, to the dimensions of matter. Instinct and intelligence were incompatible with each other, and must needs be realized along independent lines of evolution. Despite these determinations, life is creative in its nature, is, indeed, a "need of creation," and realize its creative possibilities by grafting on to the necessity of physical forces the largest possible amount of indetermination. It is this indetermination, this creativeness, which characterizes life, that cannot be accounted for on mechanistic or finalistic hypotheses.

In grounding his theory, Bergson subjects the current hypotheses respecting factors in organic evolution to a critical analysis, and seeks to identify their positive contributions to a more adequate theory of evolution. Eimer's theory of orthogenesis is accepted within the limits imposed upon it by Eimer himself, though the latter's interpretation of orthogenesis in physicochemical terms is rejected.[1] The Weismannian criticism of the Lamarckian theory of variations is accepted, with some minor qualifications, though the assumption of certain Lamarckian hypotheses that effort plays a part in evolution is taken in a special sense of the word, and made one of the principal founda-

[1] *Creative Evolution,* p. 86.

tions of his own theory. While effort in the ordinary sense of the word cannot produce a complication of structure, the development of complex structures must nevertheless be related to some sort of effort. Such effort is attributed to the given *species* undergoing the evolutionary process, and is inherent in the germ but not in the activities of individual organisms as such. Thus, aside from the occasional transmission of acquired characters "admitted to occur," Lamarckian conceptions are entertained only as they may be made to incline in the direction of a vital impetus, "an effort of far greater depth than the individual effort." [1]

Bergson likewise accepts the neo-Darwinian hypothesis in part, but reinterprets the part accepted to suit the necessities of the vital impetus. He agrees that germinal changes are the essential causes of variations, but conceives such changes, not as individual and accidental in their origins, but as developments of an original impulsion which passes from germ to germ.[2]

On the basis of this analysis, here indicated in outline, Bergson formulates the fundamental concept of his theory. This concept, in his own words, is "that of an *original impetus* of life, passing from one generation of germs to the following generation of germs through the developed organisms which bridge the interval between the generations. This impetus, sustained right along the lines of evolution among which it gets divided, is the fundamental cause of variations, at least of those that are regularly passed on, that accumulate and create new species. In general, when species have begun to diverge from a common stock, they accentuate their divergence as they progress in their evolution. Yet, in certain definite points, they may evolve identically; in fact, they must do so if the hypothesis of a common impetus be accepted." [3]

We have now the essentials of Bergson's doctrine before us, so far as it concerns our present inquiry. He has himself supplied applications of the doctrine which should make a decisive appraisal thereof possible. These pertain to the origin of certain types of instincts. He rejects both the neo-Darwinian and the neo-Lamarckian hypotheses on this problem; the former because highly organized instincts could not be produced by accidental variations adding themselves together; the latter because it assumes that "an acquired habit can become hereditary, and that it does so regularly enough to ensure an evolu-

[1] *Ibid.*, pp. 76–87. Quotations from this work have been authorized by Henry Holt and Company, the publishers.

[2] *Ibid.*, p. 85.

[3] *Ibid.*, p. 87.

tion." [1] But consistently with his general theory, Bergson concedes the correctness of the neo-Darwinian position that instincts are evolved in or through the germ, and of the neo-Lamarckian doctrine that the genesis of instinct is related to some sort of effort.

In discussing elsewhere the deftness with which *Ammophila Hirsuta* paralyzes, without killing, the caterpillar in which it lays its eggs, Bergson maintains that there is no evidence in support of the orthodox Lamarckian hypothesis respecting the acquisition and transmission of such a faculty. The knowledge implied in Ammophila's behavior cannot be expressed in terms of intelligence, but is more in the nature of sympathy, "which teaches it from within, so to say, concerning the vulnerability of the caterpillar." Instinctive knowledge of this character has its root in the unity of life, which, in the words of an ancient philosopher, is a "whole sympathetic with itself." [2]

These views are illustrated from the complex reproductive behavior of Sitaris, which is intimately bound up with the activities of Anthophora, a kind of bee. After describing Sitaris' behavior (a favorite topic in biological literature), Bergson goes on to say that "everything happens *as if* the larva of the Sitaris, from the moment it was hatched, knew that the male Anthophora would first emerge from the passage; that the nuptial flight would give it the means of conveying itself to the female, who would take it to a store of honey sufficient to feed it after its transformation; that, until this transformation, it could gradually eat the egg of the Anthophora, in such a way that it could at the same time feed itself, maintain itself at the surface of the honey, and also suppress the rival that otherwise would have come out of the egg. And equally all this happens *as if* the Sitaris itself knew that the larva would know all these things. The knowledge, if knowledge there be, is only implicit. It is reflected outwardly in exact movements instead of being reflected inwardly in consciousness. It is none the less true that the behavior of the insect involves, or rather evolves, the idea of definite things existing or being produced in definite points of space and time, which the insect knows without having learned them." [3]

Instinct, according to this hypothesis, is not evolved through experience, but is an attribute of the vital impetus.[4] In other words, it is a type of functional activity with whose origin functional activity itself has nothing to do. We have here much the same type of theory

[1] *Ibid.*, pp. 169–170.
[2] *Ibid.*, pp. 172–174, 167.
[3] *Ibid.*, pp. 146–147.
[4] Bergson suggests, however—though the suggestion is given a rather ambiguous expression—, that some special instincts of plants and animals may be susceptible of a Lamarckian interpretation. *Op. cit.*, p. 167.

as the germinal theory in its application to active adaptations, for both deny to experience or functional activity any part in the genesis of those adaptations, and both affirm that the origin of such characters has its locus in a germ-plasm which is not influenced in any representative sense by somatic activity. This agreement between the two theories pertains to fundamentals, and much the same criticisms apply to them both.

Let us take Sitaris' reproductive behavior as a test case. Bergson would be obliged to hold, on his theory, that the specific adaptations to Anthophora exhibited in this behavior issued from that part of the vital impetus which was represented by the germ-plasm of Sitaris' ancestors, and apart from any reaction on the latter's part to Anthophora or its ancestors. This hypothesis involves one of two consequences: (1) either Sitaris and Anthophora, together with the remarkably correct and detailed knowledge of the latter by the former, were virtually given in the vital impetus at the beginning of evolution, and these species were evolved, and this knowledge distributed, at the right points in space and time to make the application of that knowledge in the reproduction of Sitaris possible; or (2) remarkably correct and detailed knowledge of one species by another can come into existence, without learning, as the occasion demands, or often enough at least to guide the instinctive actions of all organisms characterized by this type of instinct.

The first alternative, when generalized, would mean that there has been no evolution at all, at least in the animal kingdom, for an extension of the hypothesis so construed to other species endowed with instincts (and all animal species are in some measure so endowed) would bring the whole domain of animal life within the scope of this alternative. Bergson's doctrine that complicated bodily structures and also the splitting-up process in evolution are determined by the *original* impetus of life is in harmony with, and complementary to, this alternative implication of the special hypothesis (pertaining to instincts) now under consideration. These several interpretations are consistent also with the general dictum that the vital impetus is the fundamental cause of variations, especially those which are passed on and produce new species. Adaptations in the genesis of which interactions with the environment are conceded to be a factor would only explain the sinuosities of the evolutionary movement, not the movement itself nor its general directions.[1]

On this construction of the hypothesis, which on the whole is the most consistent one open to us, there has been no evolution in the

[1] *Op. cit.*, p. 102.

proper sense of the term, but only a special creation which has unrolled itself in time, according to a plan largely preordained, even as to details. There is also the special and, as it seems to us, insuperable difficulty of accounting, on any construction of this hypothesis, for the vast amount of representative knowledge implied by instincts and active adaptations in general, all of which have reference to specific and often unique features of the environment. Such knowledge, on Bergson's hypothesis, is supernatural knowledge in the strictest sense of the term, for it is not acquired by any type of natural process with which we are acquainted. This, again, lends support to our general criticism that the hypothesis is in its implications equivalent to a doctrine of special creation.

These criticisms could be legitimately pressed to the point of demonstrating a *reductio ad absurdum* for the central concept of the theory. By adding together the Bergsonian interpretations of instincts from whose origin functional activity in a representative sense is said to be excluded, we should be treated to the spectacle of the species now instinctively adapted to one another, and of their later differentiations, and interadaptations, being predetermined in the vital impetus of a common ancestor. A consideration of the probable genetic relationships between present-day species, when brought into connection with these implications of the hypothesis, would mean that all the differentiations between these species, together with their interadaptations, were predetermined in the vital impetus of the one or few common stocks from which all present-day species are assumed to be derived.

It is true that Bergson regards evolution as a process of creation, and living organisms as "reservoirs of indetermination," but this, it seems to us, is radically inconsistent with his doctrine of a vital impetus, and hardly capable of being reconciled with it. The concept of an original vital impetus determining a host of specific hereditary changes is clearly contradictory to the concept of evolution as a process of creation. And Bergson has nowhere attempted, so far as we can discover, to harmonize the two conceptions. Certainly if the world of life was obliged to divide into animal and plant kingdoms; and if animal forms were obliged to develop either along the line of instinct or the line of intelligence; and if there was, in independent series, a predetermination toward the development of qualitatively identical complex structures, such as the retina; if, again, the creatures of instinct were preadapted to each other before they ever appeared in the world, the amount of indetermination in life is not as great as Bergson makes it out to be; and we should rather speak of a *created,* not of a *creative,* evolution.

On the alternative construction of this special hypothesis, Sitaris and other species possessed of similar instincts are supernaturally endowed with the representative knowledge implied by those instincts, as the occasion demands. This, too, would involve an act of special creation, but one occurring repeatedly, as the needs of organisms depending thereon might require. But this construction of the hypothesis is scarcely favored by Bergson's general theory, and Bergson himself rejects it when he says that "the instinctive knowledge which one species possesses of another on a certain particular point has its root in the very unity of life;"[1] for this unity is a derivative of the common vital impetus from which these species have issued.

While Bergson says that the knowledge of Sitaris is implicit, he means implicit as compared with conscious knowledge. It is knowledge expressed in action rather than in consciousness, but the knowledge is there, however it may be distinguished from knowledge associated with consciousness or awareness. The behavior of Sitaris with reference to Anthophora, or of *Ammophila Hirsuta* in paralyzing without killing the caterpillar in which it lays its eggs, assuredly connotes representative knowledge of some sort, and very precise knowledge at that. But this knowledge becomes supernatural knowledge on any hypothesis that entirely rules out learning or experience from the process of acquiring it.

These difficulties disappear and life becomes a tentative, contingent and, if you will, a creative process, when the vital impetus is deprived of supernatural knowledge—knowledge of such a character as, on Bergson's assumptions, to make it virtually omniscient—and made to do its work on the basis of experience or functional activity, in much the same way as does the individual organism endowed with intelligence. This does not deprive vital factors of their organizatory functions in life processes, but, on the contrary, assures those functions to them. For the endowment of the vital impetus with the vast amount of specific representative knowledge imputed to them on Bergson's theory really makes that impetus almost if not quite impotent, since such knowledge carries with it the implication that the directions of evolution, the differentiation of species, and their adaptations, one to another, were virtually given in the beginning, so that there was little or nothing left for the vital impetus to do.

What Bergson thought to be a parallel development of eye structure on independent lines of evolution was regarded by him as a crucial test of current hypotheses respecting the origin of variations, a test which turned out, at his hands, destructively for those hypotheses;

[1] *Op. cit.*, p. 167.

and also as a confirmation of his own theory that a vital impetus is the fundamental source of variations. But the so-called eye of the Pecten, the development of which was thought by Bergson to parallel that of eye structure in the vertebrates, has not been proved experimentally to serve visual functions, while plausible reasoning suggests that it serves functions of a different sort, and it is certain that the embryology and the histology of the two structures are different.[1]

Bergson's argument in connection with this assumed parallelism fails, therefore, but that may be because the case chosen by him is an unfortunate one.[2] For analogous or parallel variations have been recognized since Darwin's time, and a long list of them has now been compiled. This list includes the progressive reduction of the hind toes among the Artiodactyla, the successive development, in Pulmonata and Opisthobranchia, of double and triple genital ducts from the single genital duct originally characteristic of these groups, similar development of the heart in crocodiles, birds and mammals, and so on.[3]

We may suppose that Bergson would have dealt with any of these or other genuine cases of parallel variations as he has with the assumed parallelism in development of eye structure in the Pecten and the vertebrates. Moreover, the development of genuine eye structure, as in the vertebrate, would, on his theory, be interpreted in terms of the vital impetus, even though parallelism of eye development on independent lines of evolution could not be cited as a confirmation of his theory, nor as a disproof of rival theories. By examining his interpretation of this case the implications of his theory will be further developed, and the nature of the difficulties confronting it made more manifest.

We may say, by way of preliminary, and in support of a criticism barely indicated in our initial characterization of Bergson's theory, that he assumes the possibility of assigning the credit for specific evolutionary changes to *one* of the factors, or categories of factors, operative in all vital processes. He considers, for example, the question whether function is a derivative of structure (in the case of the eye), or *vice versa;* and although he does not return an answer to this question, he does not imply that a decision thereon is not possible in principle.[4] Likewise, in his discussion of the several hypotheses claiming that one or another of the factors operative in evolution furnishes a causal explanation thereof, each is regarded by him, either as offering a

1 Johnstone, James, *The Philosophy of Biology,* pp. 233–234.

2 Johnstone, *op. cit.,* pp. 234–235.

3 These and other examples are listed by Plate, L., *Selektionsprinzip und Probleme der Artbildung,* fourth edition, pp. 514–515.

4 *Op. cit.,* pp. 61–62.

practically complete explanation, or rejected as contributing little or nothing to an explanation. Better stated, specific evolutionary changes are attributed exclusively to one factor *or* another, but not to *all* the factors operative in vital phenomena. Thus, it is admitted that "acquired characters" may be occasionally inherited,[1] and that color-changes of the skin may be due to physical and chemical causes.[2] There is no suggestion of the possibility that the same hereditary changes may be produced by physicochemical conditions *and* functional activity, together with other factors associated with them in vital processes. The same point is more significantly illustrated, of course, by the doctrine of the vital impetus as *the* fundamental source of variations. In logical terms, Bergson is out for *monistic* interpretations of specific evolutionary changes, and does not raise the question whether the interpretations of such changes must not be pluralistic or synthetic in character.

Proceeding on this basis, Bergson goes gunning for the Darwinian hypothesis of small accidental variations, the mutationist theory of fairly large, sudden variations, the theory of direct action by the environment, the Lamarckian doctrine of the inheritance of acquired characteristics, in their several applications to the phenomenon under consideration, the development of eye structure. He is able to bag them all, and the job is done with the skill of a practised sportsman. He can do this because none of these theories makes any adequate provision for the *organization* of variations, or physicochemical processes, or effects of functional activity, as the case may be. He thereupon votes for organizatory factors—that is, the vital impetus—as *the* cause of the development in question, and appears not to have reckoned with the possibility that organization is both *of*, and limited by, functional activity, and conditioned throughout by physicochemical conditions. He takes the criticized theories on their own terms—that is, monistically or, one might even say, atomistically—, and this is all right as far as it goes. But the attitude toward them is too negative in character, for their *positive* contributions to the theory of evolution are not properly assessed. Certain minor rôles are attributed to factors emphasized by one or another of those theories, and certain processes suggested by the same theories are sucked up, as it were, into the vital impetus, but Bergson nowhere recognizes factors of *coordinate* importance with the vital impetus in the evolutionary process. In short, we have, in the hypothesis which Bergson has given us, nothing approaching a synthetic theory of evolution.

[1] *Ibid.*, p. 78.
[2] *Ibid.*, p. 74.

But let us illustrate from Bergson's interpretation of eye structure the method he employs and the criticisms we have directed against it. After emphasizing the number of elements in the structure of the eye, and their "marvelous" coördination into a highly efficient visual apparatus, he criticizes the mechanistic and finalistic hypotheses respecting the genesis of this structure, and credits it to the action of the vital impetus. The monistic character of this hypothesis is emphasized by the ascription of the alleged parallelism in development of eye structure on two independent lines of evolution to portions of the vital impetus assumed to be equivalent in those two lines.

The action of the vital impetus is likened to an invisible hand pushing its way through iron filings, the final result of this movement being the coördination of the filings according to a certain definite form, the shape of the hand. This result cannot be explained as the action of the filings on each other, as the mechanists would hold, nor as the ordering of these elementary actions according to a plan of the whole, as the finalists claim. It is the outcome of one indivisible act, that of the hand pushing its way through the filings. In logical terms, the hand and its movement are the cause, the arrangement of the filings the effect; and "the whole of the effect is explained by the whole of the cause, but to parts of the cause parts of the effect will in no wise correspond." The relation of vision to the detail of eye structure is "very nearly that of the hand to the iron filings that follow, canalize and limit its motion." [1]

What more could the most thoroughgoing believer in special creation demand? The vital impetus, which, be it remembered, does not itself evolve, is likened to the hand (a quite determinate object) which has merely to push itself through matter, so to speak, in order to provide itself with a suitable structure. The shape, size and inner plan of this structure were obviously determined when the impetus itself was determined, and this was at least as far back as the beginning of evolution. That is the reason for the assumed parallelism of eye development in the vertebrate and the mollusc; it was an equivalent vital impetus, a ghostly hand common to the two series, that embodied itself in matter, that took on flesh and blood, if we may so speak. To propose a figure for ourselves, the *mould* of the later structure was given at the beginning, and it only remained to pour matter therein, or push the mould through matter, for the work of creation to be complete.

Is it not clear that we have here a type of hypothesis against which Bergson himself is never weary of inveighing—a sort of mechanistic

[1] *Ibid.*, pp. 94–95.

hypothesis, that is, and one of a particularly crude sort, according to which the whole of the cause produces the whole of the effect? On this interpretation, functional activity has played no rôle, except a purely instrumental one, in the evolution of eye structure and the coördinations between its constituent elements. These were all virtually predetermined in the vital impetus before any beginnings of eye structure appeared. Where, then, has there been an evolution, and especially a creative evolution? It is true, as we have before observed, that Bergson conceives life as a process of creation, as "a continuous creation of unforeseeable form," but how is that reconcilable with so deterministic a concept as the vital impetus? The surprising thing here is that there should be *an all but express* adoption of special creationism, together with an *express* affirmation that life is creative in its nature. There is, in other words, an insistence upon two hypotheses which are radically opposed to each other.

The vitalistic hypothesis in this form thus turns out to be an equivalent of special creationism, a special creationism which is in no wise mitigated by assigning to the created functions or tendencies certain tasks to be performed. The most confirmed predestinarian assigns the same sort of tasks: they are in both cases *appointed* tasks which can be performed in only one way. If instincts adapting one species to another were given in the original "whole sympathetic with itself," then instincts have not been evolved, and neither have the species which are endowed with the instincts. If, as Lloyd Morgan says, such relationships obtain "throughout the whole range of life," [1] then *no* species have been *evolved*—they have merely *appeared*. The vital impetus has merely cast them up from the depths where they already existed. And if the organism taken as a whole has not evolved, neither have the component parts of the organism. They have merely been cast in moulds preëxistent in the vital impetus from the beginning.

Notwithstanding these fallacies, serious though they be, Bergson has made valuable contributions to the theory of evolution. Perhaps the most valuable is to be found in his destructive criticism of current hypotheses—physicochemical, Lamarckian, neo-Darwinian, direct actionist—that make no adequate provision for the organization of specific processes in the organism; and of finalistic hypotheses, which put organization *outside* the category of natural processes. These criticisms are very effective, and no successful rebuttal of them has been forthcoming thus far. On the more positive side, his contribution consists of the demonstration that organizatory factors distinct

[1] "Are Meanings Inherited?" *Mind,* N. S., Vol. XXIII, 1914, p. 177.

from physicochemical conditions are operative in vital processes, and that these factors are within, not outside of, vital processes. Of positive value, too, is his doctrine that organizatory factors are endowed with more or less specific capacities from the beginning.

But the theory is badly stated, and, for that reason, not as adequately supported as it might be. This comes from overemphasis on the rôle played in evolution by vital factors in their *primordial* form, and his failure to recognize the coördinate importance of the rôles played by other categories of factors, and, in particular, physicochemical processes and functional activity. The latter category must be largely assimilated, as we saw, to the general category of organizatory factors operative in vital processes, so that the weakness of Bergson's general theory comes, in part, from his failure to assign to the vital impetus the property of learning from or proceeding on the basis of functional activity. He claims too much for his vital factors in one direction, that of their original capacity to organize matter, and too little in another, that of learning from or being guided by functional activity. He therefore has to endow his vital factors with supernatural knowledge of such a sort and extent as virtually to constitute omniscience, and with the specious creative power which such omniscience implies. His theory therefore inevitably issues in a sort of special creationism, and becomes, owing to the assumption of *life's* creative possibilities, self-contradictory.

Let us attempt to identify in a general way the rôles played in the genesis of complex eye structures by the several categories of factors that may have coöperated therein. This should enable us to define the type of functions served by the vital impetus in an evolutionary process of this character, illustrate the functions served by coördinate categories of factors in such processes, and at the same time prepare the ground for a brief consideration of evolutionary processes along definite lines, of which the case before us is a striking example.

We may leave out of account in relation to this case the part played by mental (or sub-mental) factors, though these factors may have a good deal of significance therefor, since visual functions are organically related to other mental functions and must have evolved in connection with those functions. Cultural factors must of course have played a minor rôle in this particular evolutionary process, except perhaps in the case of our own species. Organizatory factors would seem to have been the more significant category of vital factors in this process. We may also take for granted, without a specific analysis thereof, the functions of time and space, the media, so to speak, in which

natural processes of all types occur, and the rôles possibly played by logical and mathematical entities, in this process.

We must assume that the organizatory factors which entered into life processes at the beginning of the evolutionary movement were endowed with the capacity of constructing all the types of organic structure that have since appeared in the world or that will appear hereafter, together with the functional characteristics that go with all these types of structure. The qualifications of this general assumption which would be necessitated by a consideration of mental, cultural and other categories of factors in vital processes are here taken for granted. Another qualification of a speculative sort may be indicated. It is possible that vital processes are from time to time invaded, as it were, by organizatory factors from the outside, and capacities or complications of a vital order added to those already inherent in the given life processes. Whether or to what extent this is the case we have no way of determining.

Assuming for the purpose in hand that the vital factors operative in evolutionary processes were all given in a latent form at the beginning of the movement, we may say that the specific capacities and limitations of these factors are revealed up to this time in the various types of organic structure and functional activity that have appeared in the world. Like matter, energy and every other category of factors in nature, we have to infer their properties from their actual manifestations. These cannot be foretold in advance of such manifestations.

Coming now to the case under consideration, we must assume, subject to the qualifications stated, that in the beginning there was a potentiality of highly developed vision such as we find in the vertebrates, together with a capacity for the gradual construction of a visual apparatus requisite to the realization of that potentiality, provided physical and chemical conditions should permit of it. But the specific structure of the visual apparatus was not predetermined in that potentiality and that capacity. That apparatus had to be, so to speak, a function (in the mathematical sense) of the vital factors in which that potentiality and capacity were inherent, and also of the physicochemical conditions which had to be satisfied in the construction of a functionally efficient visual apparatus. If the vital factors are not to be endowed with supernatural knowledge, they must have operated, in the work of constructing such a visual apparatus, on the basis of experience or functional activity. More particularly, the host of active adaptations between the various constituent structures of the visual apparatus must have been gradually constructed by the organizatory fac-

tors involved, working under the guidance of experience or functional activity.

The physicochemical conditions involved in the process seem to have played a specially important rôle in determining the outcome. A careful analysis of the visual apparatus in vertebrates almost forces upon one the conclusion that no other type of structure could have served highly developed visual functions such as characterize this phylum. If that be so, we are justified in concluding that the problem which confronted the vital factors endowed with the potentiality of highly developed vision, and the capacity for constructing the requisite visual apparatus, could be solved in only one way, and that, to speak somewhat figuratively, they were able to find that solution. On these assumptions, the definitely directed variations issuing in the complex eye structure of vertebrates were *prescribed* by the physicochemical conditions that had to be satisfied, and not by the vital factors operative in the process. But we must add that the given vital factors were able to follow this prescription to the end.

So far as anatomical and physiological evidence can supply an answer to the problem, it tends to support the hypothesis here proposed. The evidence goes to show that each element of eye structure has a specialized function, the want of which would more or less seriously impair the visual function as a whole, and that the several elements of this structure are pretty strictly prescribed by the specialized functions which they serve.

The structure of the eye had to be of such a character that an enormous number of localized sensations could be produced, and interpreted as indicating the color, shape, size, motion and other features of external objects. For these functions, a mosaic of nervous elements was necessary, the excitation of which could induce sensations varying in quality, extent, sequence and other properties. The types of visual discrimination characterizing the human species and probably other vertebrates required that these nervous elements should be quite numerous, specific and elaborately organized. The larger part of these specifications is represented by the structure of the retina. This structure and its constituent elements are, to all appearances, strictly prescribed by the specific functions which they serve.

The refracting media of the eye can be similarly interpreted. These media must be such that the stimulus can be applied to the retina in an order corresponding to the external object. The lens, the cornea and the humors of the eye fulfill this requirement. These media, which at the surface of refraction are approximately spherical, must be centered on the same axis if the image of the retina is not to be dis-

torted, but approximately proportional to the seen object. This requirement is fulfilled in the so-called "optic axis," on which the refractive system is centered.

"Accommodation" is the process whereby vision is focussed on near objects. This process is not the same in different orders of vertebrates; in amphibia and reptiles the lens moves forward, while in mammals the front but not the rear surface bulges forward, making the lens more curved. The optical power of the eye must be increased through some such process, if clear vision of near objects is to be secured, and the two processes specified would seem to be the only possibilities in this direction.

We are not qualified to give an answer to the question whether this complicated system of mechanisms is the *only* system whereby the retinal image could, in the different circumstances where the visual function comes into play, have been properly formed, but, apparently at least, there is, in every part of the mechanism, so close a conformity to physical laws, that such an inference would be warranted. This inference is further strengthened by the fact that, when different solutions of specific problems were allowed by these laws, we find different solutions actually adopted, as when, to secure accommodation, amphibia and reptiles move the lens forward, while in the case of mammals, it is given a greater convexity.

We have to speak less confidently of the solutions prescribed by chemical conditions, for the reason that the chemical problems relating to vision have not been as fully worked out as have the physical, and especially the optical, problems. But so far as present evidence goes, it tends to support the hypothesis here proposed. Although it appears probable that the effect of light on the retina is of a chemical nature,[1] we cannot yet say whether the rods and cones have the only specific chemical composition which would have fitted them for their special functions. But these several functions are probably specific for the various histological elements involved. Various evidence goes to show that the cones have little sensitiveness to dim light and little power of dark-adaptation, but that these functions are served by the rods. The cones according to this evidence are the apparatus for color vision and for distinct visual sensations. Moreover, to account for specific light and color sensations, the current hypotheses assume there are different sorts of receptors or sensitive substances in the retina. These assumptions are supported by a mass of experimental evidence, including the facts just alluded to, the phenomena of color-blindness, etc. All this evi-

[1] It is doubtful if this is true of the cones. See on this point and on an alternative hypothesis, E. B. Holt in *The New Realism,* p. 312.

dence tends to show that the structure is *prescribed* by the corresponding function, and that if elements of a given structure be wanting or defective, the corresponding functions must suffer.

We can speak more confidently of a *necessary* connection between the *number* of the retinal elements and the analytic power of vision. Apparently each of these elements is a distinct sensitive structure, corresponding, on the one side, to specific components of the stimulus and, on the other side, to specific color and light sensations. Only on this assumption can we account for the delicate visual analysis of objects in the center of the visual field, where a large number of retinal elements comes into play, or for the far less refined analysis of objects in the peripheral parts of the visual field, where a much smaller number of retinal elements, especially of the cones, comes into play.[1]

If now, as the available evidence tends to show, the structure of the eye and its constituent elements are very strictly prescribed by the visual functions to be served thereby, there is no justification for the hypothesis that this structure was predetermined by the vital factors operative in its development. As aforesaid, the potentiality of highly developed vision and the capacity for constructing the requisite visual apparatus must be attributed to those factors; but, unless those factors are to be endowed with a supernatural knowledge of the means whereby that capacity could be utilized in making that potentiality an actuality, we must assume that those factors operated on the basis of experience or functional activity, in building up and coördinating the constituent elements of the apparatus in question, and that they were able to discover and utilize the specific chemicals and energies from which that apparatus had to be constructed. This hypothesis involves the assumption that the given vital factors have been endowed with something akin to human intelligence, but this assumption is unavoidable on a systematic analysis of the facts in this case. The assumption that vital factors generally are so endowed has already been elaborated and defended against criticism, and need not be examined anew in the present connection.

The case which Bergson regarded as a crucial test of current hypotheses as to the factors in organic evolution, and as a decisive confirmation of his own hypothesis, means something quite different on an analysis such as ours, and lends support to a different hypothesis. On our showing, Bergson has underestimated the importance of physicochemical factors in organic evolution, the obvious conformity of eye

[1] The foregoing account of visual structures and functions is adapted from Ladd, G. T., and Woodworth, R. S., *Elements of Physiological Psychology*, pp. 186–196, 325–326. The interpretation here based on that account is of course our own.

structure to physical and chemical laws being quite inexplicable on his theory of the vital impetus as *the* prime factor in the development of this structure. He has also evidently underrated functional activity as a factor in evolution, the exclusion of it from his explanation of this case making it necessary for him to endow the vital factors involved with a species of supernatural knowledge, surely a less acceptable proceeding than the incorporation of the Lamarckian principle in the account of this case would be.

We may, then, agree with Bergson that the vital impetus, or primordial vital factors, represent in a potential form the functional characteristics of organisms that may appear in the world, and the capacities for building up from inorganic substances the structures requisite to the realization of these potentialities; but we must disagree with him when he allows those potentialities and capacities to determine in advance the precise organizations of matter through which they are to be expressed. There is contingency in the evolution of the structures requisite to the realization of specific potentialities, even when those potentialities are certain to be realized, since not always nor perhaps even generally would a given potential function be limited to one or a few forms; while the assumed functional potentialities may, as Bergson suggests, be obliged to split up and seek their issue along divergent lines of evolution. And there is contingency in the realization of the functional potentialities as such, since they may not always be successful in finding any structure, any organization of physicochemical substances, which can serve them.

Orthogenesis and Parallelisms in Evolution

This examination of Bergson's doctrines, and in particular our analysis of the evolution of eye structure, might serve as an introduction to the discussion of orthogenetic or definitely directed series of variations alleged to be revealed in the records of the evolutionary process. This subject is so complex, however, and involves so many controversial questions, even as regards the facts to be accounted for, that we shall have to restrict our treatment of it to some general observations and suggestions respecting certain of its more significant features. The evidence supporting the general hypothesis is quite extensive, and a large number of biologists have accepted it in one form or another. But there is great diversity of opinion as to the causes of orthogenetic variations, and because, as might be expected, the evidence is interpreted on the assumptions respecting the factors in

evolution severally accepted by the biologists believing in orthogenesis. Thus, various selectionist, Lamarckian, physicochemical and vitalistic hypotheses have been proposed to account for the facts assumed to demonstrate orthogenesis in evolution.

We shall assume for the purposes of our very brief consideration of the subject that there have been definitely directed lines of variation in the evolutionary movement, and that, as seems altogether probable, there have been many parallel developments of structure and function on independent lines of evolution. Our general theory, as elaborated in the consideration of Bergson's doctrines, would then be applicable in the interpretation of variations ordered in these ways. This theory, if valid, would compel us to conclude that vital factors, functional activity and physicochemical processes have all been involved in such series of variations. Definitely directed variations in any given case would thus be accounted for by showing how these might have resulted from specific combinations of factors falling within the categories just specified. That is what we have done in the case of eye structure, which almost certainly represents the culmination of a long series of variations along a definite line. Similar analyses could be made of other cases. Such analysis is of course highly theoretical in character, but that is the only type of analysis which can reach problems of this nature. Needless to say, analysis of this type must always have reference to the facts relative to the problems dealt with, and be corrected as often as necessary in the light of newly discovered facts bearing on these problems.

Parallelisms in development of structure would always be functions (in the mathematical sense) of specific factors falling within the general categories we have distinguished and elaborated. Physicochemical factors would sometimes *prescribe* such parallelisms in development, given vital factors capable of directing the several lines of development exhibiting such parallelisms. We could very well conceive vital factors on divergent lines of evolution possessing potentialities of highly developed vision, and capacities for gradually building up the visual apparatus requisite to the realization of these potentialities, and all obliged, because of the physical and chemical conditions of highly developed vision, to express those capacities and potentialities in practically the same way. It may turn out of course that the Pecten has a genuine eye structure, in which case the illustration here cited as hypothetical would have an existential value.

Doubtless a detailed examination of the rôles played by physicochemical factors in evolution would reveal many parallelisms in devel-

opment which were virtually prescribed by those factors. Many such parallelisms, more or less inexact, it is true, seem to have been determined by surface tension, gravitational tensions and pressures, the specific properties of the chemical substances entering into vital processes, and so on. General similarities in environmental features to which organisms must adapt themselves would account for other parallelisms. Mental traits in the most diverse groups of organisms are quite similar, in a general way, as the work of Jennings and others has shown, and many of these similarities have reference to common environmental features by which the evolution of those organisms has been conditioned. It is not implied, of course, that the environment produces such mental traits. Mental or sub-mental factors distinct from the environment must on our analysis be assumed. But the evolution of mental traits and much of their specific characters have a manifest reference to the environment. That is obviously true of the perceptive faculties of the young chick, for example, and indeed of all hereditary responses to environmental stimuli.

Similarities in vital factors would also appear to determine many parallelisms of development, given the requisite environmental conditions. The development of sexuality in the higher plants and animals and the widely distributed capacities for regeneration of mutilated parts, to cite only two significant cases, would apparently have to be thus accounted for. The many crude as well as occasional exact parallelisms in the evolution of human culture, where borrowing of cultural traits was out of the question, would have to be referred primarily to similar complexes of mental traits in the component individuals of the several societies exhibiting such parallelisms,[1] though of course conditioned by factors of other categories.

These are to be taken merely as illustrative applications of our theory to this group of problems. They will suffice, perhaps, to indicate the bearing of orthogenesis and parallelism in evolution on our general theory, and the applicability of the theory itself in the interpretation of those phenomena. It might be suggested, in conclusion, that progress in dealing with this group of problems could be much facilitated by making a more inclusive list of orthogenetic and parallel lines of development, and analyzing them systematically in terms of the various factors assumed, on different hypotheses, to be operative therein.

[1] See Wissler, Clark, *Man and Culture*, Chap. XII, for an interpretation of the general pattern common to all cultures, in terms of hereditary behavior traits. Ogburn, W. F., *Social Change*, pp. 90–102, may be consulted for a partial (and surprisingly long list of inventions and scientific discoveries each of which was made independently by two or more individuals.

The Vitalistic Interpretation of Ontogenesis According to H. Driesch

Our analysis takes on a somewhat different aspect when we turn to the vitalistic theory of ontogenesis elaborated by Driesch. According to this theory, two categories of primary factors are operative in vital processes: (1) physicochemical substances, and (2) entelechies; the former regarded as the means of organogenesis, the latter as organizing agencies which utilize these means in building up organisms and their constituent structures. Functional activity is conceived to be a manifestation of entelechy, when not identified with specific physicochemical processes. Its phylogenetic significance, however, is problematic on Driesch's theory, though an evolutionary rôle of some importance is provisionally conceded to this factor.

Driesch's formal definition of entelechy presents many points of similarity to our concept of vital factors. By this definition, entelechy has nothing in common with energy, for it "lacks all the characteristics of quantity," while energies are "quantities, and relate to phenomena which have quantity among their characteristics."[1] It operates by *suspending* physicochemical reactions, and not by inducing different reactions from those found in the inorganic world. "Entelechy," says Driesch, "*is order* of relation and absolutely *nothing* else."[2] Because it possesses the faculty of temporarily suspending inorganic becoming without at the same time being in the nature of an energy, it is *the* non-physicochemical agent.[3] Moreover, entelechian action is a type of "unifying or individualizing causality," while physical and chemical reactions fall under the type of "singular or additive causality."[4] Finally, Driesch distinguishes several sorts of entelechies, corresponding somewhat to the categories of organizatory, mental (sub-mental) and cultural factors into which we subdivided our vital factors.

The two doctrines also offer much the same account of the factors in individual development. One of Driesch's proofs of the autonomy of life is drawn from an analysis of the "historical basis of reacting," representing, roughly speaking, the acquired adaptations of the organism as a whole to its environment. As Driesch puts it, this historical basis of reacting is developed through the action of the medium on the organism, as, for example, in the acquisition of the language habits of one's social group. A machine could not be the basis of such a de-

[1] *The Science and Philosophy of the Organism*, Vol. II, pp. 168, 169.
[2] *Ibid.*, Vol. II, p. 169.
[3] *Ibid.*, Vol. II, p. 180.
[4] *The History and Theory of Vitalism*, pp. 198 ff.

velopment, for a machine is "a typical arrangement of parts built up for special purposes," and does not originate in "contingencies from without."[1] The elemental agents which direct this development are termed psychoids. These are distinguished from the entelechies which form the body,[2] but all fall within a broader category of entelechian agents. The similarity of these conceptions to those developed or implied by our own analysis should be obvious.

But the two theories differ radically as to the factors involved in the genesis of hereditary variations, and particularly as regards the rôle played therein by functional activity. Driesch concedes only a limited influence to functional activity in this process, and of a largely hypothetical character at that. He says that "congenital histological adaptedness may be regarded hypothetically as due to an inheritance of adaptive characters which had been acquired by the organism's activity, exerted during a great number of generations."[3] Driesch also notes that a few plants in which modifications are induced by environmental changes retain part of these modifications for several generations after a restoration of the original conditions.[4] It is granted, too, that purely physiological adaptations, as immunity, for example, *might be* inherited, and that adaptive congenital structures might also be due to such inheritance.[5]

The most important class of characters actually or provisionally ascribed by Driesch to the so-called Lamarackian factor is adaptive congenital structures, and these, as noted, are only hypothetically ascribed to that factor. Moreover, this group of characters is of subordinate importance on Driesch's theory, for he asserts that differences in organization, especially in types of organization, and in the degrees of complication therein, "are independent of histological adaptation and adaptedness."[6] The positive side of this assumption comes out in his statement that the unknown principle in descent must be correlated with entelechy,[7] and a species of entelechy which, as we shall see, does not operate on the basis of experience or functional activity.

This largely negative attitude of Driesch toward the phylogenetic significance of functional activity is intimately related to his conception

[1] *The Science and Philosophy of the Organism,* Vol. II, p. 77.
[2] *Ibid.,* Vol. II, p. 82.
[3] *Ibid.,* Vol. I, p. 290.
[4] *Ibid.,* Vol. I, pp. 279–280.
[5] *Ibid.,* Vol. I, p. 281, footnote.
[6] *Ibid.,* Vol. I, pp. 290–291.
[7] *Ibid.,* Vol. I, p. 295.

of the rôle played in development by entelechy. Entelechian agents, according to Driesch, have produced the different sorts of harmony exhibited in the development and functioning of specialized parts of the organism that are not intimately dependent on one another. Even where the processes in the organism are of the statical-teleological type, entelechy has created the machine-like basis upon which they are organized.[1]

The harmonies so produced are of three kinds, namely, causal harmony, functional harmony and harmony of constellation. The latter is shown by the fact that at the end of ontogeny there is a unified organism, in spite of the "relative independence of the single events leading to it." Causal harmony, in turn, is demonstrated by the fact that the formative actions in morphogenesis are always provided with the conditions requisite thereto, or, in other words, by the "unfailing relative condition of formative causes and cause-recipients." Functional harmony is indicated by the unity of organic function, and is, indeed, only a descriptive term whereby this unity of function is designated.[2]

Now, in attempting to show *how* entelechy builds up the organism and its harmonies, Driesch makes but little use of the Lamarckian principle, believing, apparently, that the discredit suffered by the doctrine of the inheritance of acquired characteristics had rendered such a principle unavailable for this purpose. The result is that entelechy, regarded as the directive agent in morphogenesis, but learning nothing from experience (except in individual action), is here beset with the greatest difficulties, as Driesch himself admits. It is significant that these difficulties are largely obviated in the one case where an appeal is made to experience, namely, the action of entelechy in building up an historical basis of reacting.

These difficulties are so crucial for a proper appraisal of Driesch's theory, that his estimation of them should be stated in his own words. "The problem," he says, "becomes very complicated as soon as we turn from the facts to the 'how,' as soon as we inquire the meaning of the primary faculties of those entelechies in which an historical basis does *not* play *any part at all.* We indeed are in a rather desperate condition with regard to the real analysis of the fundamental properties of morphogenetic, adaptive, and instinctive entelechies: for there *must* be a something in them that has an analogy not to knowing and willing in general . . . *but to the willing of specific unexperienced*

[1] *Ibid.,* Vol. II, p. 151.
[2] *Ibid.,* Vol. I, pp. 108–110.

realities, and to knowing the specific means of attaining them. And we are by no means able to understand such a specified primary knowing and willing in even the slightest degree. . . .

"Without doubt it is at this point that vitalism encounters its greatest difficulties. It is here that so many make up their minds that they cannot accept vitalism as a theory at all. They would be inclined to accept the autonomy of life as far as psychoids are concerned, as far as the historical basis of reacting, *i. e.* secondary knowing and willing, comes into account, but they feel unable to accept autonomical teleological agents unpossessed of these secondary faculties." [1]

Nevertheless these difficulties, great as they are admitted to be, are surmounted by the bold assumption that there *are* such teleological agents not possessed of experience. "It is my firm conviction that we cannot avoid the admission of vitalistic autonomic agents possessing no experience, *i. e.* no 'secondary' faculties, and yet endowed with *specific* knowing and willing: indeed, as far as morphogenesis and physiological adaptation and instinctive reactions are concerned, there *must* be a something comparable metaphorically with specified knowing and willing, but without experience." [2]

Consistently with this view, Driesch holds that entelechy lies at the root of inheritance,[3] and a species of entelechy, let us remember, that is incapable of learning much, if anything, from functional activity, that is, from its interactions, in the ancestral organisms, with the environment. The material basis of inheritance accounts only for the materials which are to be ordered; [4] the ordering itself is the work of an entelechy which learns nothing of these materials from its own activity in connection therewith.

Let us now come to grips with this theory of entelechy, and see whether it will stand the test of a rigorous logical analysis such as we propose to apply to it. Excepting the species of entelechy which builds up historical bases of reacting, the theory must mean that entelechy, an immaterial factor, has the most perfect prevision of the material conditions on which it works, or at least a sufficiently correct prevision thereof, to enable it, prior to and apart from any experience, to build up the myriad combinations of matter and energy which the countless organisms that have existed, or may hereafter exist, in the world represent. Further, since entelechy must, on the theory, have existed

[1] *Ibid.,* Vol. II, pp. 142–143.
[2] *Ibid.,* Vol. II, p. 143.
[3] *Ibid.,* Vol. I, pp. 224–227.
[4] *Ibid.,* Vol. I, pp. 227–228.

before organisms were evolved, it had all this knowledge before it had any contact with matter.

Moreover, it has learned as little from failure as from success. It has learned nothing, for example, of the selective process, and of the destructive rôle it plays in organic evolution. Not only, therefore, must entelechy have foreseen the specific structures which were possible to it, it must also have had a *prevision of the precise effects, cumulative though they have been, of natural selection in determining the specificity of these structures.* For the action of morphogenetic, instinctive and other adaptive entelechies refers, in large part, to specific structures turned out by evolutionary processes, including the processes of natural selection.

Now, natural selection, being the result of the interaction between the organism and its milieu, is a highly contingent process, considered with reference to the organism. For the organism *does* enjoy a measure of autonomy, and, so, exercises some sort of choice in respect to alternatives presented to it by the environment; and the milieu itself is, with reference to most organisms, a highly independent variable. We then have the spectacle of entelechy foreseeing the results of a doubly contingent process: the action of the organism itself with reference to the environment; and the influence of the environment with reference to the organism. Indeed, the contingency of the process is of a higher power than that, as the milieu always includes a great number of organisms, and their various actions with reference to the particular organism subjected to the selective process are of a highly contingent character. Moreover, the milieu includes variable inorganic factors, such as moisture and temperature, whose variations may be of the most vital moment to the organism.

Entelechy must on the theory foresee this highly contingent process and its results. We have here an absurdity, but one to which the theory surely leads. There is the additional absurdity of entelechy building up innumerable organisms which cannot survive this selective test, when it knew in advance that they could not, or, again, of its building up entire species which it knew would be extinguished in the end.

These absurdities are emphasized when we attempt an application of the theory to the congenital adaptations between organisms of different species, or between members of the same species. The hypothesis here becomes equivalent to the hypothesis of a vital impetus as advocated by Bergson. If entelechy learns nothing from functional activity, then it must have known in advance just what species were

to appear in the world, or it could not have constructed the adaptations which fitted these species to one another. Since such adaptations obtain throughout the whole range of life, it must have had a foreknowledge of *all* the species that were to exist, or at least all to which there was to be adaptation on the part of other species. If entelechy knew that these species were to appear in the world, then the existence of particular species was predestined from the beginning, and there has been no evolution in their case, but only a predetermined order of development in time. Not only that, but the successions of species in distinct lines were correlated in advance, were synchronized, that is, since adaptations between species imply interactions between them at the same time, and not at two different periods of time. A cat-mouse adaptation (assuming there is such) implies the existence of cats and mice at the same time, and not, say, in different geological epochs. The habitats of all species were determined, too, be those habitats geographically large or small. Cats and mice and all other interadapted species must occupy the same areas if their adaptations are to come into play. The evolution of the organic world as a whole is therefore strictly determined in advance of any evolution.

We have, then, not only a miraculous foreknowledge by entelechy of the various species of organisms which were to appear in the world, together with the properties of the chemicals and energies constituting their material environment, but the history of the organic world was strictly determined from the beginning. Vitalistic autonomy on this showing is quite illusory. If organisms had to be just what they were and are, if their specific characters, and their spatial and temporal position in the vital economy were all determined from the beginning, then entelechy has nothing creative about it in the sense of being able to choose alternatives that might be presented to it. Its vast prescience could be no more than a contemplative knowledge without any influence whatever on action. These implications are, of course, directly opposed to the spirit of Driesch's doctrine, but they are, notwithstanding, logical consequences of that doctrine as he has formulated it. The doctrine would therefore appear to be a self-contradictory one.

Our analysis could be repeated for both histonal and intra-cellular adaptations, and we should arrive at a similar result. This criticism would be qualified by Driesch's provisional acceptance of the Lamarckian hypothesis as accounting for histonal adaptedness, but this qualification is not a specially important one in view of the fact that such characters are for him of a very narrow range [1] and not coterminous with life itself. We suppose Driesch would hold that every semi-

[1] *Op. cit.*, Vol. I, pp. 290–291.

autonomous part of the organism—organ, tissue, cell, etc.—is presided over by a separate entelechy, albeit there would be, on his hypothesis, a sort of master-entelechy that orders all these entelechies and gives unity to their work. But, so far as phylogeny is concerned, they learn nothing from functional activity, and are not guided, therefore, by any sort of experience. Entelechy must then have had a foreknowledge of all the diverse types of functional specialization exhibited by organisms now or in the past, as well as of the specific arrangements of matter corresponding to these specializations; for otherwise it could not have built up the myriad connections between parts—adaptations, that is—which have been requisite to unity of function and structure in the various species of organisms that have appeared in the world.

As before, the results of a highly contingent process are foreseen, since selection involving many independently variable factors partially determines the specificity of the semi-autonomous parts of the organism (Roux), as well as of the organism as a whole. As before, also, the evolution of specific parts must be strictly predetermined as to time, if adaptations between them are to fit, and these parts must also be predetermined, and most precisely, as to their successive positions in space, if multicellular organisms are to result. For none of the entelechies presiding over cells or other semi-autonomous parts could learn anything from functional activity as to these matters, and they could not therefore communicate any such knowledge to other entelechies, even if these latter could learn from them. The entelechy which presided over the whole organism could no more learn from functional activity than could the subordinate entelechies, and the assumption of such a master-entelechy would therefore not avail to alleviate the foreordination of the whole congeries of parts, as regards time, space or specific structure.

Finally, as in the case of organisms strictly so-called, entelechy could play no creative rôle, since everything is predestined to occur just as it does, seeing that entelechy possesses a most perfect foreknowledge of what is to take place in the organism. We thus have the same contradiction as before. Entelechy is rendered powerless, so to speak, by having too much knowledge granted it. Not learning anything from its own activity, it must have a perfect knowledge beforehand of the matter which it is to organize, and of all the organs, tissues, cells, etc., which it is to adapt to one another. But such a knowledge implies that all these structures are predetermined from the beginning, and are not, strictly speaking, the result of an evolutionary process.

These criticisms by no means imply that the doctrine of an entele-

chian factor in vital activity is a fallacious one, or that Driesch's analysis in support of that doctrine is to be depreciated. That would be to underestimate the value of Driesch's positive contribution to the theory of life, and his destructive criticism of physicochemical and other mechanistic interpretations of life. Our own criticism of the concept of entelechy as Driesch has left it is offered as a contribution to the further analysis of entelechy which he has himself called for. We must not, in justice to his work, forget that his principal aim has been to refute hypotheses which deny that autonomic agents are operative in vital activity, and not to work out a complete theoretical analysis of this category of factors.

Now, the first step in the further analysis of entelechy should be to redefine it and rid it of contradictions. This we can do by adopting certain fragmentary analyses presented by Driesch himself, and giving these analyses a wider application than he has been disposed to grant them.

What needs to be done is to make entelechy more human, as it were, which means that it will also be made less divine. It needs, more particularly, to be relieved of the property of "willing specific unexperienced realities, and of knowing the specific means of attaining them." In other words, all entelechies should be demoted to the rank of the *entelechia psychoidea* which build up historical bases of reacting, entelechies that know only through learning and act on the basis of the knowledge thus gained. Entelechy being thus rendered more modest, it might then claim to exercise a real, and not an illusory, power. It would not have a foreknowledge of matter and of all the specific organic characters destined to appear in the world, a foreknowledge that implies the strictest predetermination of vital activities; but it would learn from its action, in a way analogous to *our* learning from our actions, and it would be able, we might then grant, partially to determine its action in the light of the knowledge thus acquired.

The theory would gain, moreover, from the abolition of the arbitrary division of entelechies into those that learn from activity, and those that have no capacity for such learning. Driesch's view that any general capacity for adaptation to environmental changes would imply vitalistic causality [1] would then be consistent with the concept of entelechy, as, without a revision of the concept, it would not, adaptation implying a capacity for both learning and profiting from experience. A revision of the concept in the way here suggested would render the same service for the view that, should the inheritance of

[1] *Op. cit.*, Vol. I, p. 272.

acquired characters be proved to occur, it might be regarded as a new evidence of vitalism.[1] Lastly, the inconsistency of the theory with the principle of evolution would be obviated, and the view that entelechy, and not matter, is connected with the "unknown principle concerned in descent" [2] could be justified. These fragmentary, disconnected analyses would all be consistent with our revised concept of entelechy, and might, indeed, have served originally as points of departure for the definition of that concept.

Further justification of the revision here proposed may be derived from our former analysis of anti-Lamarckian hypotheses respecting the origin of active adaptations, and the demonstration that these hypotheses all involve consequences which must be deemed quite fatal to them; and from the conclusion growing out of the same analysis that no theory respecting the origin of such adaptations is tenable which excludes functional activity in a representative sense from a share therein. It was there shown that all such theories suffer from a common defect, namely, their inability to account on their several assumptions for the specific adaptations of the organism to its environment, or of the component structures of the organism to one another. That demonstration applies with critical force to Driesch's hypothesis.

This hypothesis in addition labors under a special difficulty, one shared with the germinal hypothesis as advocated by Weismann and his followers. This lies in the consequences of the assumption—explicitly made in Driesch's theory—that the organism and its constituent structures possess a *representative* knowledge of the specific and often unique environmental conditions to which they are severally adapted, that is not derived from functional activity in connection with those conditions. As pointed out before, such knowledge is supernatural knowledge, and any hypothesis which implies its existence is clearly in conflict with the fundamental postulates of modern science.

Organizatory factors as we have conceived them may claim many advantages over the rather crude vitalistic factors assumed by Driesch, Bergson and other vitalists. All vital activity, according to our concept, is of much the same nature as the activity we observe every day in the animal (including the human) world. It is tentative, experimental, laboriously building up procedures of action, gradually developing new and more serviceable knowledge, often suffering defeat or utter failure, but frequently achieving victory or a balance of success, and proceeding then to a mastery over obstacles not hitherto overcome

[1] *Ibid.* Vol. I, p. 277.
[2] *Ibid.,* Vol. I, p. 295.

or even encountered. The whole process becomes a highly contingent one, the results of which are quite unforeseeable. A real evolution, a creative evolution, if you will, is seen to be not only possible in principle, but as bound to occur. Organisms are not miraculously endowed with adaptations to the environment, including organisms of other species, but acquire those adaptations through conflict or coöperation with that environment, in much the same way as the individual human being, dog or paramecium becomes adapted to its environment. We are not obliged, under this conception, to meet such embarrassing questions as, for example, how a specific congenital response to a definite stimulus (as in animal instincts) could possibly have been produced apart from prior reactions to that stimulus; how all the tissues of the body could have become so wonderfully coadapted if the coöperative functioning of the corresponding tissues in ancestral organisms contributed nothing to that achievement, and so on.

We can understand, too, which we could not on Driesch's analysis, why organic evolution has taken the slow pace it manifestly has. For if entelechy had so perfect an acquaintance with matter as Driesch has assumed, it should have been able, had not a knowledge so great condemned it to impotence, to organize matter into the complex structures of to-day in a relatively short period of time. The refractoriness of matter would have retarded the process, no doubt, but has not a limited capacity for acquiring a knowledge of matter also retarded the process? Certainly entelechy could, like a chemist, have made much greater progress in synthetizing material substances for its purposes, had it possessed a complete knowledge of these substances at the start. Is it not clear that any sort of entelechy we can imagine must have acquired its command over matter in much the same way as the chemist acquires his—by working with it, and discovering what combinations thereof were possible and useful? And must not our entelechies, like the chemists, have been limited in the beginning by the incompleteness of their knowledge, a knowledge, however, which has been extended through their efforts to utilize matter?

We must, in conclusion, deal with what Driesch regarded as particularly conclusive evidence of entelechian action not based on experience or prior functional activity. We may summarize this evidence in his own words, together with the conclusions which he believes are supported by it. He says that "there are many phenomena in morphogenesis, notably all the phenomena akin to restitution of form, which occur in absolute perfection even the very first time they happen. These processes, for the simple reason of their *primary per-*

fection, cannot be due either to 'learning' from a single adaptation, or to accidental variation." [1] When the matter is put in the way Driesch puts it, the inference has to be such as Driesch himself draws. But Driesch has not reckoned with an alternative and, as it seems to us, the only tenable interpretation of these facts. He does not consider the possibility that the capacity for regenerating a certain part is evolved at the same time as the part itself. That is the hypothesis we must accept unless we are prepared to believe that the evolution of species manifesting this property was predetermined in advance of that evolution. We cannot suppose that the newt's power of regenerating its leg, for example, was given in advance of the newt's own evolution, as Driesch's hypothesis implies, for that would be to suppose that the newt itself was given in advance of its evolution. If, as we have shown, functional activity must be conceded to have an influence of a representative or specific character in the genesis of the organism, then it has the same sort of influence in the genesis of the capacity for the regeneration of specific parts, in organisms which manifest such a property. The two go together.

Driesch has himself insisted upon the similarity between, if not the equivalence of, restitution and normal reproduction, and we only need to assume that capacity for reproduction may be, to a certain extent, a somatic or fractional as well as a germinal property, to understand this group of phenomena. On our analysis functional activity has been a factor of primary importance in the evolution of histological and other active adaptations, such as are involved in specific somatic structures, for the regeneration of which there may be a capacity; and, if that be so, the capacity for regenerating such specific structures was evolved when the structures themselves, with their active adaptations, were evolved. The property of reproduction has, so to speak, become a *somatic accompaniment* of all such structures, and is not something *added to them* from the outside, as the occasion demands. Put a little more accurately, the property in question is a limited, but often highly serviceable, somatic property, as well as a germinal property. There is of course a high order of organizatory capacity manifested in regenerative processes, but it is manifested in connection with characters in whose genesis functional activity had a part. It has become specialized by and through the same processes whereby the structures *with which it is linked up became specialized.* [2]

[1] *Op. cit.,* Vol. I, p. 218.

[2] See *supra,* pp. 141–143, for a further discussion of regeneration considered as evidence against the modified form of Lamarckism there advocated.

Other Vitalistic Writers

Our general theory has now been sufficiently elaborated, perhaps, for its implications respecting the primary factors in vital processes to be made clear. The position occupied by the theory with reference to other types of theory treating the same problems has also been clearly defined. This being the case, but little would be gained in the way of further elaboration by examining the views of other writers. However, certain other types of vitalistic theory may be briefly canvassed with a view to gleaning from them something further in the way of contributions to a theory of vital phenomena.

Reinke's theory of dominants presents many points of similarity to Driesch's doctrine of entelechy.[1] These dominants are specialized for the various parts of the organism, arranged in some sort of hierarchy, unendowed with energy of any kind, and charged with the organizatory functions of the body, as are Driesch's entelechies. They also, like the latter, are elemental factors in nature, coördinate in importance with matter and energy. But they function to a greater extent than do entelechies through the construction of genuine bodily mechanisms. Indeed, Reinke likens the organism as a whole to a machine, though he does not believe that all vital processes can be represented in mechanical terms. Consistently with this conception, dominants, in building up the organism, work analogously to the technician in constructing a machine. Reinke's assumptions, that dominants work unconsciously, that they may influence one another in their actions, and that their existence cannot be proved or represented but must be postulated, further differentiate his theory from Driesch's concept of entelechy.

Features of the theory which, from our standpoint, give it an immense advantage over Driesch's doctrine are (1) its assumption that hereditary adaptations are developed in the same way as individual adaptations, that is, by reactions to environmental stimuli; and (2) its more critical attitude toward the hypothesis that some types of hereditary variations may be directly induced by environmental stimuli. On the other hand, both agree in relegating selection as a factor in evolution to a secondary position, on the ground that it has no power to originate an adaptation, but only to eliminate organisms not adequately equipped with adaptations.

Finally, Reinke assumes that psychical factors, radically different both from physicochemical factors and from dominants, are operative in the organism, and partially determine its behavior. He holds that

[1] *Einleitung in die theoretische Biologie*, second edition.

the relationships between body and mind are interactionist in character, and that, although we cannot now explain these relationships, our daily experience demonstrates their reality. It is asserted, however, that external factors, which, in combination with internal factors, produce all vital phenomena, are wholly material and energetic, while the internal factors are only partly so.

Our own position respecting these several conceptions can be readily deduced from our appraisal of views held by other writers. Briefly put, we should say that vitalistic conceptions of life are as capable of proof or disproof as are mechanistic conceptions, and by similar methods, for both types of conceptions rest on complicated inferences from facts bearing on the questions at issue between them; that vital factors *may not* work unconsciously, since they are demonstrably endowed with intelligence or something analogous thereto; that external factors affecting the organism are not all in the nature of matter and energy, since other organisms not constituted exclusively of matter and energy, and non-physicochemical cultural factors derived from the past (in the case of the human species), are important categories of environmental factors. More affirmative would be our position toward the view that adaptations to environment are not directly induced by environmental conditions, but that they originate in the reactions of the organism to those conditions; that selection plays only a negative rôle in the genesis of adaptations; that organizatory factors (dominants) should be distinguished, at least for purposes of analysis, from mental, or sub-mental, factors (Reinke's psychical factors); and, more generally, that the organic world must be sharply distinguished from the inorganic world, because it embraces categories of factors not found in inorganic nature.

An interesting theory of life has recently been propounded by J. S. Haldane, the eminent British physiologist.[1] Beginning with a vigorous attack on physicochemical conceptions of life, and insisting as well on the inutility of vitalistic conceptions, he proposes to regard the organism as something *sui generis,* but nevertheless as having its place in a comprehensive whole of experience termed personality or God, conceived to be the only ultimate reality. The inorganic, the organic and other types of being are all implied in this ultimate reality, and are therefore not ontologically, but only logically, distinct from one another. But the organic is a higher order of reality than the inorganic, and science must aim at eventually interpreting the physical world in terms of the biological conception of the organism. At present, however, we can see no way of reducing what is distinguished as

[1] *Mechanism, Life and Personality.*

the inorganic, or even many details of what is observed in organisms, to terms of this more valid conception.

Haldane combines in his doctrine a largely negative valuation of mechanistic and vitalistic conceptions of the organism, one based almost wholly on methodological considerations; and a positive theory of reality (including life and matter) based on considerations of an entirely different order, these largely derived from the idealistic metaphysics of Kant and Hegel. In his elaboration of this doctrine, Haldane seems not to have considered the question whether, from the ontological standpoint, a pluralistic rather than an absolutistic theory of life may not be supported by the criticism of mechanistic and other one-sided conceptions of the organism. Into this question we cannot here enter, as it would involve a systematic examination of the idealistic metaphysics underlying Haldane's theory, from which we are precluded by limitations of space. We will only repeat what has been reiterated so often in this discussion, that if the chemicals and energies entering into the economy of the organism can exist in an inorganic state, which is unquestionable; and if, further, these chemicals and energies cannot wholly account for their own organization in living organisms, then one or more additional categories of elemental factors must be at work in the organism.

We would remark, further, that it appears not to contribute much toward an understanding of reality to assume that all such categories are in the nature of logical distinctions only. For they are in nowise arbitrary distinctions forced by the human mind on reality, but may be said, on the contrary, to be forced by reality on the human mind. As Haldane himself says, we can now see no way of reducing the inorganic to the organic, although he anticipates, and rightly, we think, that biological conceptions of some sort will eventually be utilized in the interpretation of inorganic phenomena. Nor, on his view, can the organic be reduced to the inorganic. It would seem well to admit that distinctions of this sort which we can see no way of overcoming have their counterpart in reality, and not assume *a priori* that this is not the case.

The literature dealing with the so-called mind-body problem is very extensive, and we have been unable on account of space limitations to deal with more than a limited portion of it. An additional reference to a recent work on the subject,[1] however, may be permitted. McDougall in this work arrives at conclusions favorable to vitalism, or what he prefers to term animism. Of his specific contributions to the

[1] McDougall, W., *Body and Mind.*

discussion of the general problem, only a few can be singled out for notice. His acute criticism of psychophysical parallelism in its various forms is probably his most noteworthy contribution. This criticism resulted in the demonstration of numerous incomplete correspondences between processes in the brain and nervous system, on the one hand, and various types of mental processes, on the other hand.[1] The evidence thus indicated supports the position taken in a preceding chapter, that the two series of phenomena, when compared by the method of concomitant variations, lend no support to mechanistic or parallelistic conceptions. Especially noteworthy is McDougall's readiness to concede that psychical research, so-called, is dealing with phenomena of the very greatest significance for this group of problems, and his belief that evidence has already been discovered by investigators in this field which is irreconcilable with mechanistic conceptions of the human mind.

The last work which we shall here cite is a remarkable treatise by Edmund Montgomery [2] embodying the results of extensive researches into the bearing of vital phenomena on various philosophical problems, particularly the problem of substance, regarded by him as the most fundamental of these problems.

Montgomery claims to have demonstrated that the living organism is the only thing discoverable in nature which preserves its identity in change, and therefore the only thing known to us deserving to be regarded as a substance. The organism, according to his doctrine, is a system of "extra-conscious, power-endowed existents," which is symbolized to the organism itself by states of awareness referred to itself, and to outside observers by sensations and perceptions, which are also states of awareness, but referred to the external world. In support of this conception Montgomery presents a trenchant epistemological criticism of materialism and idealism in their various forms, showing that all types of these doctrines hypostasize states of awareness—sensations or ideas, as the case may be—which are evanescent, forceless, and altogether incapable of accounting either for themselves or for changes inferred to occur in nature. The efficient causes of change both in conscious states and in nature at large are extra-conscious, power-endowed existents which constitute both the matrix of conscious states and the moving forces back of natural phenomena. The organism acts teleologically, according to Montgomery, and "becomes progressively organized through functional interaction with the

[1] See *supra*, pp. 348–349.
[2] *Philosophical Problems in the Light of Vital Organization.*

medium." Lamarckism is therefore in principle accepted. Inconsistently with these views, the organism is spoken of in other passages as a complex of chemical activity.

The feature of special interest in this work, from our point of view, is the progress evidenced thereby toward a resolution of the physical and the mental as ordinarily conceived into realities of a more enduring and efficacious sort. The analysis bears a strong resemblance to our own analysis in support of the doctrine that sub-physical and sub-mental factors are operative in all phases of our experience, whether designated as mental or physical. From our viewpoint, Montgomery's analysis is incomplete in that he fails to distinguish within his "extra-conscious, power-endowed entities," two radically heterogeneous categories of entities, termed by us sub-physical and sub-mental, respectively, a distinction which, as we showed, is requisite to any comprehensive and consistent account of our experience.

This criticism could be enforced by a consideration of Montgomery's questionable attacks on the principle of the conservation of energy, and his insistence that mental processes are incapable of exercising any influence on the movements of the organism, positions which are tacitly abandoned by him in other parts of the same work. But such a consideration would lead us into detail which would here be out of place. Despite the serious errors thus barely indicated, Montgomery's acute criticism of current philosophical doctrines is of fundamental importance, and we may expect it to be increasingly influential on philosophical thought in the future.

CHAPTER XV

CONCLUSIONS AND IMPLICATIONS

WE may dispense with any final recapitulation of our analysis, as we have from time to time summed up the results thereof, and applied these in the further elaboration of a theory of life and the primary factors operative therein. In this our final chapter we shall only review the salient features of the analysis, develop its more significant implications for the biological sciences, and examine possible objections to the methods and assumptions of the analysis, that have not been adequately considered hitherto.

LOGICAL PRINCIPLES UNDERLYING THE ANALYSIS

The more salient features of the analysis may be reviewed in relation to the logical principles underlying the analysis as a whole. One of these principles is that we must take facts as we find them, and not ignore, discount or distort any of them in our theories respecting relationships between the facts. This principle comes close to, although it is not exactly equivalent to, the methodological postulate of radical empiricism. It agrees with the latter in its contention that a valid scientific hypothesis is capable of being tested by experience, while differing therefrom in maintaining that, if we are to formulate a comprehensive and consistent account of experience, inferential analysis of the data immediately supplied by experience, often of an extremely complicated type, is necessary; and that such analysis may demonstrate the existence of relationships in nature which cannot be directly experienced.

An important corollary of this principle is that *all* the facts bearing on a given problem must be duly considered in the treatment of that problem, even though they appear to be *remote* from the situation in which the problem itself is centered. The methodological significance of this corollary will appear presently, in the discussion of a second logical principle underlying our analysis. Here we shall only assert that facts seemingly unrelated to the situation in which a complex problem centers are likely to have a vital bearing on that problem, and that, accordingly, great care should be exercised in identifying the

types of data which bear on such problems and the hypotheses entertained with respect to them.

A second logical principle underlying our analysis is that the various relationships inferred to subsist between the data of experience must be compatible with one another. This principle we have employed as a criterion in the evaluation of various hypotheses respecting the primary factors in vital phenomena, and have shown by means of it that many of these hypotheses contain implications repugnant to established laws or principles applying to the same phenomena. We showed, for example, that many such hypotheses are incompatible with the principle of evolution, regarded as the outcome of complex and highly contingent processes of interaction between semi-independent variable factors, and therefore as not determined in advance of those processes. This test of the hypotheses in question by the criterion of self-consistency was, concordantly with our first principle, always conducted with reference to representative *data* bearing on these several hypotheses.

A third logical principle, one closely related to the preceding principles, was employed in the classification and interpretation of the data bearing on our problems. This principle may be considered under two different aspects. In one of its aspects it means that factors which are indissolubly associated together in a given situation must not be regarded as acting separately *in that situation,* but that changes in the situation must be attributed to *all* those factors combined. This does not imply, however, that the given situation may not, as such, be broken up and resolved into the various factors which combined to produce it. The chemicals and energies combined in a living organism, for example, may all exist afterwards in an inorganic state, though we are unable to say what becomes of the non-physicochemical factors in an organism at the time of its dissolution.

In its other aspect the third principle means that unlike features of natural phenomena must be kept distinct, and that categories or classifications based on common features of such phenomena must not be extended to include factors or entities not exhibiting those particular features. The application of our third principle in this aspect to the problem of psychophysical relationships resulted in the conclusion that mental processes could not be reduced to terms of neural and other bodily processes, and because mental processes do not as such exhibit the features common to, and constituting the classificatory basis of, bodily processes.

A further application of this same principle resulted in the conclusion that the physiological processes of the organism cannot be com-

pletely expressed in physicochemical terms, for it was found that qualitative, quantitative, spatial and temporal relationships subsist between the physicochemical processes involved, and that relations of these specific sorts do not characterize physicochemical processes as such. These relationships were therefore deemed to connote the presence of non-material factors in physiological activity, a category of factors designated, according to the functions subserved by them, as organizatory in character.

By a somewhat different method, though based on the same principle, it was shown that organisms are sharply differentiated from inorganic substances in incorporating sensory or physical elements of sorts not attributable to the latter, those, namely, constituted or indicated by the motor and organic sensations. A less difficult application of the same principle yielded the conclusion that organisms are further differentiated from inorganic substances by processes or functions termed psychical, a position which is granted by all schools of thinkers on this question, except certain radical empiricists. It was shown also that these psychical functions are responsible for a type of behavior that has no parallel among inorganic events; and that behavior of this type cannot be completely accounted for in terms of the matter and energy associated with it. This evidence was deemed to justify the proposition that psychical (or sub-mental) factors ontologically distinct from the associated physical (or sub-physical) factors are fundamental determinants of animal behavior.

Other applications of the same principle resulted in distinctions between cultural and mental factors, as well as between mental factors involving consciousness or intelligence, and organizatory factors operating in the body which are not demonstrably endowed with these properties. The latter distinction was made somewhat problematical, however, because evidence is not available whereby to arrive at a final conclusion as to its validity. Separate categories of logical and mathematical entities and of space and time were justified on the same principle, no one of these appearing to be characterized by attributes qualitatively identical with attributes constituting the classificatory basis of any other category.

By application of the other aspect of this principle, factors of these various categories were shown to be variously synthetized in the same concrete situations, wherefore the specific characters of, and changes in, such situations must be attributed, it was insisted, to factors of the various categories severally involved therein, and not to selections from these factors. We wish to speak here, however, of the problems involved in the analysis of such situations into their determining fac-

tors. These problems are often exceedingly difficult, owing to the complexity of the syntheses in question. The mind-body relationship is such a problem. So difficult has it been, for the reason stated, that investigators have been unable hitherto to work out any generally acceptable solution of it. Our own program for clearing up the problem is based on the assumption that the mental and the physical are always in the nature of fusions between sub-physical and sub-mental factors. These categories do not violate the principles of classification here stressed, as do categories employed by other hypotheses respecting this problem; and they are capable at the same time of accounting for the various types of the mental and the physical, so-called, presented in experience. But factors of other categories enter into this situation, as we showed, and for these due allowances were made.

These examples will serve to illustrate the application of our third principle in the analysis and classification of factors in vital phenomena. The principle is related to the first principle in implying what the latter explicitly affirms, that we must take facts as we find them; and in explicitly affirming what that principle implies, that we must not force classifications upon facts which are repugnant to the facts themselves. It is related to the second principle, in emphasizing a special application of the latter, namely, that the various categories by which we interpret our experience must be severally self-consistent, and not include factors repugnant to the given categories, or complex syntheses incorporating elements from distinct categories of factors.

An auxiliary principle utilized in evaluating physicochemical conceptions of life is represented by the method of concomitant variations. The results attained by this method were deemed to be of especial significance, because by them were confirmed the results of the analysis to which the concepts and categories of those hypotheses were subjected, on the basis of the logical principles previously discussed. By the use of this method physicochemical hypotheses were brought to the test of the immediate data bearing thereon, while their evaluation by the other criteria involved a complicated inferential analysis of the basic concepts and categories employed by those hypotheses.

A postulate underlying the entire analysis is that natural phenomena are wholly determined by factors within the phenomena themselves, and in no degree by factors or powers external to the given phenomena. Concordantly with this postulate, the various categories of factors, which by the analysis we concluded to be determinative of vital phenomena, were assumed to be *natural* factors within these phenomena and normally operative therein.

This postulate was employed as a special criterion whereby to appraise various hypotheses respecting the origin of hereditary variations. The results showed that many such hypotheses involve consequences repugnant to this postulate. Since the advocates of all these hypotheses themselves accept this postulate, these hypotheses must perforce be repudiated by their several advocates, should our applications of this criterion be found, on examination, to be substantially accurate ones.

We showed that no type of anti-Lamarckian hypothesis could square itself with this postulate. No form of the germinal hypothesis could account, as we saw, for the specific adaptations of the organism to its environment, or of its component structures to their environments, since such adaptations all *represent,* and must be partially defined in terms of, specific, oftentimes unique and irreproducible, features of the environment; and, on the germinal hypothesis, this could be accounted for only by assuming some sort of preëstablished harmony between the variations issuing in the given adaptations, and the features of the environment to which they have reference. It was shown also that the occurrence of the requisite variations at the proper time and in the proper order could not be accounted for on this hypothesis, save on the assumption of a directive agency from the outside, even when unfit variations and the eliminative rôle of natural selection were generously allowed for.

The hypothesis of parallel induction was shown to involve similar consequences, maintaining, as it does, that the same stimulus acting on two wholly different structures—soma and germ cell—can induce equivalent somatic modifications in parent and offspring, respectively, a proposition which would be unintelligible save on the supposition that some outside agency, or at least some agency for which the hypothesis itself does not allow, determines coördinations of this type. Any hypothesis that *adaptive variations* are due to direct action of environmental stimuli on the germ cells implies the same sort of agency, for it assumes that such a stimulus acts on one structure, the germ cell, in such a way as to determine adaptive modifications in a totally different structure, the soma of the offspring.

We saw, too, that the interpretation of active adaptations on the non-representative transmission hypothesis implies that some external coördinating agency must determine the equivalence between the hereditary adaptations of the offspring and the acquired adaptations of the parental organism, since chance representative modifications of the germ-plasm through the given somatic activities could not occur in

the proper measure and order to account for the genesis of active adaptations characteristic of living organisms, and especially of complex adaptations.

Since, as pointed out, the advocates of these hypotheses would accept the postulate of non-intervention in vital processes by outside agencies, they must grant the untenability of their several hypotheses, or show that we have erroneously deduced the implications of these several hypotheses.

Anti-Lamarckian hypotheses could also be shown to violate one or another of the logical principles employed by us as criteria of the validity of scientific hypotheses. They have all failed to deal adequately with the *facts* of hereditary active adaptations. This is because the advocates of these hypotheses have not fully realized that all such adaptations connote specific bonds between specific entities and must be defined in terms of these entities. They make no adequate provision, therefore, for the coöperation of these entities or factors in the establishment of such bonds. Only hypotheses assuming that functional activity is a factor in the genesis of active adaptations can account for the specific bonds connoted thereby. This defect of analysis also denotes a failure to resolve the complex syntheses represented by active adaptations into their specific determinants. Moreover, insofar as these several hypotheses assume that physiological processes are ultimately physicochemical in character, there would be the added failure to resolve those processes into physicochemical reactions *and* the actions of organizatory or other vital factors. All the anti-Lamarckian hypotheses except the vitalistic do make such assumptions. Where these hypotheses are not explicitly materialistic, they are at least *atomistic,* as regards their assumptions. For they make no provision for the *organization* of the discrete processes in phylogenesis assumed by them to be fundamental. The rejection of functional activity as a factor in phylogenesis is a special though very important case of such failure. For we saw that functional activity represents the coördination of specific factors in vital phenomena, and that it is therefore to be regarded as a manifestation of organizatory factors at work in the organism.

Still another respect wherein anti-Lamarckian and, for that matter, other types of hypotheses test out negatively by one of our criteria is in the tacit assumption—an assumption which explains many defects of those hypotheses—that the several factors or processes of the organism need not all be responsible for the changes which take place therein. This assumption underlies the direct action and germinal hypotheses in their several forms, as well as vitalistic hypotheses

which deny or minimize the part played in evolution by functional activity or physicochemical factors, as the case may be. For these several hypotheses purport to account for hereditary changes in the organism by a partial selection from the factors or processes admitted to be operative therein. This assumption is not so obviously involved in the physicochemical hypotheses, since these hypotheses purport to account for the organism and its activity in physicochemical terms. Being *monistic* physicochemical conceptions of the organic, no other factors than the physicochemical are recognized. The particularistic forms of the hypothesis, however, do purport to account for specific changes in the organism by *less* than the entire ensemble of physicochemical factors involved therein.

Recurring to the postulate of non-intervention from the outside, it may be pointed out that Driesch and Bergson come pretty near to assuming the invalidity of this postulate, maintaining, as they do, that vital factors (entelechy, the vital impetus) are endowed with knowledge in no sense derived from the situations to which that knowledge has reference and in relation to which it functions. This assumption, as we showed, brings their theories in conflict with the fact of evolution itself, and indeed with other assumptions of their own theories. But we found that their hypotheses could be put on a sounder footing and made to yield invaluable contributions to the theory of evolution by purging them of this assumption, together with the anti-Lamarckian assumptions which logically precede and underly it.

The theory worked out in the light of the postulate and principles here expounded, and of the critical analysis to which current hypotheses were subjected by employing this postulate and these principles as criteria, purports to be a synthetic theory of life and its evolution, including the special case of human behavior and experience. This theory has of course been presented in outline only; its own appraisal, correction and elaboration is a task for the future. The postulate and principles stated, applied systematically to this group of problems, yield a pluralistic theory of life and of human experience, which maintains that matter, energy, mental capacities, organizing agents and cultural factors (especially in the case of the human species) all play fundamental rôles therein. But we found, on a closer analysis, that matter, energy and mental capacities must be resolved into two distinct categories of factors, denominated sub-physical and sub-mental, respectively; that organizatory factors must operate on the basis of experience or functional activity, not miraculously acquired knowledge; that logical and mathematical entities condition the behavior

and experience of human, if not other, animal organisms; that space and time enter in some sense into the constitution of living, as of non-living, things, besides being the medium in which they all have their existence.

Assumptions of the Analysis

The analysis as a whole is based on certain assumptions that are not altogether acceptable to positivistic thinkers on philosophical questions, and is likely on this account to encounter criticisms from that quarter. These assumptions have already been defended to some extent in connection with the discussion of specific problems; but it will be well to state them here and present such further justification of them as seems called for in a work of this character. The assumptions in question are:

(1) There are imperceptible entities in nature.

(2) There are entities in nature which are presented in experience only in combination with entities of other categories. By such combination the characters of the given entities are rendered inaccessible to direct observation. *Pro tanto,* the relationships among entities so synthetized are not given in immediate experience. Entities and relations of these sorts are identified through an analysis of the data wherein they are represented.

(3) There are necessary connections in nature.

(4) There are realities in nature which are not definable in terms of entities or complexes thereof coming within our immediate experience, but must be conceived as capacities, powers or possibilities which represent such entities in an *undeveloped* or *inactive* form.

The first two assumptions have been defended in the body of the text, and little in addition need be said about them here. They are stated now because of their logical connections with assumptions (3) and (4), and because their justification affords also a partial justification of the latter assumptions. Some remarks on the first two assumptions, however, will be in order.

Our analysis of organic phenomena, particularly of animal behavior and experience, resulted in the conclusion that these phenomena and their various interrelationships cannot be understood save on the assumption that various classes of imperceptible entities are essential components or determinants thereof. The various lines of evidence adduced in support of this assumption need not be recapitulated in relation to the problems here under consideration. We shall only remark that the conclusion in question is not wholly repugnant to

positivistic ways of thinking; for it could be held, on various types of positivistic doctrine, that the imperceptible entities deduced by our analysis, while not conclusively proved to be determinants of organic phenomena, are possibly or probably such, or that they are at least convenient conceptions whereby, together with other conceptions, such phenomena may be compendiously summarized. It is quite possible, we are ready to concede, that the analysis purporting to demonstrate the subsistence of these entities has been vitiated by serious errors; and in this case, of course, the categories deduced by us must be revised or abandoned, according as criticism of the analysis may show to be necessary.

Similar remarks would apply and need not be detailed in relation to the second assumption, namely, that certain classes of entities and relations thereof are not given in immediate experience, but are rendered inaccessible to direct observation by entities of other categories wherewith they are, in experience, always synthetized.

The third assumption, that there are necessary connections in nature, must be examined here at some length, having been taken for granted in the discussion thus far. This assumption has been hotly debated among philosophers since Hume so ably criticized it, and it is a moot question in philosophy at the present time. An examination of the many diverse positions on this question would hardly be appropriate in an inquiry such as ours; and we shall deal with it, accordingly, only by indicating some of the grounds, however inadequate, for our own position with respect to it.

Pearson, Mach and other modern exponents of the positivistic position on this question hold that necessary connections in nature are not demonstrable, but that however deep our investigation of natural phenomena may penetrate, we never discover anything save sequences of sense-impressions that can be described but not shown to be necessary. "Law in the scientific sense," says Pearson, "only describes in mental shorthand the sequences of our perceptions. It does not explain *why* those perceptions have a certain order, nor *why* that order repeats itself; the law discovered by science introduces no element of necessity into the sequence of our sense-impressions; it merely gives a concise statement of *how* changes are taking place. That a certain sequence has occurred and recurred in the past is a matter of experience to which we give expression in the concept of *causation;* that it will continue to recur in the future is a matter of belief to which we give expression in the concept of *probability.* Science in no case can demonstrate any inherent necessity in a sequence, nor prove with absolute certainty that it must be repeated.

Science for the past is a description, for the future a belief; it is not, and has never been, an explanation, if by this word is meant that science shows the *necessity* of any sequence of perceptions."[1]

"Scientifically, cause, as originating or enforcing a particular sequence of perceptions, is meaningless—we have no experience of anything which originates or enforces something else. Cause, however, used to mark a stage in a routine, is a clear and valuable conception, which throws the idea of cause entirely into the field of sense-impressions, into the sphere where we can reason and reach knowledge."[2] Elsewhere, in discussing the alleged necessity of planetary motions, Pearson says that "the necessity lies in the world of conceptions, and is only unconsciously and illogically transferred to the world of perceptions."[3]

Pearson does not pronounce definitely as to the source whence comes the routine of sense-impressions, but holds that it "lies either in the field of the unthinkable beyond sense-impressions, or else in the nature of the perceptive faculty itself."[4] He inclines, however, to the latter alternative.[5] It is suggested by him that the perceptive and reflective faculties are coördinated as a result of natural selection, "so that the former accepts what, in wide limits, can be analyzed by the latter."[6] The function of the reflective faculty, more explicitly stated, is to reduce phenomena of all types to brief formulae or laws, or, somewhat differently expressed, to describe "wide ranges of phenomena by simple laws."[7]

Conceptions similar to these are held by Mach, James and other positivistic thinkers, though naturally there are significant differences between the views of writers accepting this general standpoint. James, for example, attaches a somewhat different significance to "conceptual experiences" and to cause-effect relationships from that ascribed to them on Pearson's doctrine, while neither Mach nor James interprets sense-impressions subjectivistically, as does Pearson, but regards them as neutral components both of minds and of physical objects. Perhaps a more detailed exposition of these and other differences among positivistic writers on the questions under consideration may, for our present purposes, be dispensed with.

The implications of this general position that bear specially on the

[1] *The Grammar of Science,* second ed., p. 113.
[2] *Ibid.,* pp. 128–129.
[3] *Ibid.,* p. 134.
[4] *Ibid.,* p. 115.
[5] *Ibid.,* pp. 137–139.
[6] *Ibid.,* p. 104.
[7] *Ibid.,* pp. 105–106.

substantive problems of our inquiry have been considered in connection with those problems, and these need not be examined anew, though some incidental reference to them will be entailed in the examination of other features of the general position. We shall limit the discussion here to the question whether this doctrine seriously undermines our third assumption, namely, that there are necessary connections in nature; and to the further question—if this assumption shall appear to be tenable—whether radically different types of sequences in sense-impressions do not connote differences in the types of causal factors involved therein.

We should begin by insisting that it is intelligible to ask the question whether changes in a given group of phenomena are not determined by something (to use a neutral term) which is constituted or indicated by these phenomena; or, put differently, whether later stages in a series of phenomena are not the necessary resultants of factors at work in earlier stages of this series. If this question is at all intelligible, it is more reasonable to answer it in the affirmative than in the negative. The implications of either answer to this question, however, can be fully developed only on the basis of a recognition that any occurrence in nature appears to be reducible to one or more series of natural phenomena or events that always occur according to relatively uniform sequences, when any members of the given series occur.

Now, a negative answer to the question, granting the validity of the latter, is tantamount to the assertion that the relatively uniform connections between phenomena of particular sorts are merely coincidental, whereas the chances of coincidences occurring successively in the measure denoted by these uniform connections are infinitesimal. Therefore, the relatively uniform connections between specific sorts of phenomena are made wholly unintelligible and indeed miraculous, when it is maintained that these connections are not necessary connections. This argument is of course not a new one, but we are adopting it because it does not appear to us ever to have been successfully challenged.

The foregoing discussion does not imply that the innumerable sorts of necessities indicated by the various (relatively) uniform connections in nature can be *fully* understood. Many connections can be understood in terms of other connections. Chemical behavior, for example, can be partially explained in terms of the behavior of electrons in the atomic units involved, but the behavior of the electrons themselves cannot be explained to the same extent, if at all. Similarly, a man's behavior can be understood in terms of his hereditary phys-

iological and mental constitution, his social and cultural environment, and the various conditions of his geographic environment, as these have interacted to produce his personality. But why his hereditary mental endowment, for example, functions as it does in development, or how it interacts with the associated physicochemical processes in the brain and nervous system, we cannot now, if ever, understand.

These observations could be generalized in the proposition that an exhaustive analysis of natural processes of any sort whatever, while it might account for many of the connections therein—and particularly for the grosser, more complex connections, in terms of more specific, elementary connections—, will nevertheless arrive at connections which are quite inexplicable. It would seem, indeed, that none of the connections between the irreducibly simple elements in nature are susceptible of explanation at all, possibly because these elements are, so to speak, first causes, beyond which we are unable, in our investigations, to penetrate. But this hardly affects our main contention here, namely, that uniformity of connections in nature implies the necessity of those connections, and because any other interpretation renders them wholly unintelligible, and even miraculous.

We shall have to leave the matter in this form, since we are not so much concerned to refute the positivistic position on the question at issue, as to indicate some of the grounds for our own antipositivistic position. It may be remarked, however, that good positivists would not be obliged to reject any part of the substantive analysis presented in this book because of the underlying assumptions on this question, since they could readily interpret in their sense the various types of causal relationships treated by the analysis.

The further question, whether radically different types of sequences in sense-impressions do not imply differences in the types of determining factors involved, has of course been answered by us in the affirmative. Assuming that complex groupings of natural phenomena and their various types of sequences can be accounted for in terms of causal determinants involved therein, we have undertaken to identify the various sorts of factors or entities which must be assumed in order thus to account for vital phenomena, and particularly for animal behavior and experience. We showed, for example, that no sensation can be wholly accounted for in terms of other sensations or sense-impressions, but that all such are resultants of interactions among factors not given, in a pure form, in sensation or other phases of our experience. We showed also that the behavior of animal organisms with reference to distant (and, in large part, *imperceptible*)

objects cannot be accounted for in terms of sensation alone, or of the sub-physical factors in sensation. These conclusions strike at the heart of the positivistic position, at least the version of it elaborated by Mach and Pearson; and it does not seem possible successfully to rebut them, except by falling back on the dogma that natural phenomena uniformly connected together are not related through necessity any more than are phenomena that are never, or hardly ever, connected together.

We concluded, of course, that many sorts of entities are active in nature which are imperceptible to the senses. These we shall not recount here, but would remark that the evidence of their existence would all challenge the fundamental position of positivism, particularly of the orthodox type. Still, these categories of entities could be accepted by all good positivists as at least valid *conceptions,* if nothing more, just as all positivists agree that atoms, molecules, energy, etc., while not perceptible to the senses, are more or less useful conceptions whereby to summarize sense-phenomena.

We shall now discuss briefly our fourth assumption, namely, that there are existences in nature that are not explicitly definable in terms of entities or complexes thereof coming within our experience, but must be designated as powers, capacities or possibilities representative of such entities in an undeveloped or inactive state. The validity of this assumption depends on that of the assumption just considered, that later stages in a uniform series of events are determined by earlier stages in the same series, or by factors indicated thereby. Only on the fourth assumption, in turn, can we account for changes in an isolated system of natural phenomena, since the *phenomena* given therein at one time could in themselves hardly determine phenomena in the same system at a later time partially unlike themselves. Such changes must be ascribed in the main to the quiescence of some of the properties in the causal factors involved, which were active in earlier stages, and to the activity of other properties which were quiescent in those stages.

If the validity of our third assumption be granted, evidence for the correctness of the fourth assumption is readily cited, and is indeed inexhaustible in its variety and extent. Take, as one case, the development of the human individual. The baby brings into the world with it a complex something, to use a neutral term, from which an adult personality, with its numerous ideas, habits, interests, abilities, etc., is developed, given the necessary environmental conditions. The baby does not possess these characters at birth. All of them will at the adult stage have been partly determined by environmental factors.

Yet they are not *added* from the outside. They are a product and, in some sense, a synthesis of hereditary endowment and environmental factors interacting together. How are we to define this hereditary endowment? We are obliged to do so in some such terms as capacities, powers, possibilities, which lead to the later ideas, interests, abilities, etc., but which are *not* these, at birth.

Much the same observations apply elsewhere. Carbon, to take another case, may exist in an inactive state, chemically speaking. Yet we know that a given quantity of carbon may enter successively into a great number of different compounds and, theoretically, into tens of thousands of such compounds. Most other elements also enter into different sorts of chemical compounds, though not in so many as does carbon. Likewise, electrons of the same kinds are the dominating components of all the chemical elements, so that only a fraction of the nature of an electron is expressed by way of the chemical element that happens to incorporate it. Organizatory factors, in turn, do not fully express their nature in any or all of the physiological activities that occur in the world at a given time, for, together with other factors, they will determine continuously thereafter, as they had determined before that time, more or less different types of physiological activity.

How shall these various entities be defined, so as to allow for the diverse types of existence and the diverse types of events, which they may severally constitute or determine, but which are not all constituted or determined by them at any given time?

They cannot be defined in terms of the sense-impressions which they determine at any given time (when they determine sense-impressions at all), for most of the factors that determine sense-impressions may determine diverse sorts thereof, but not all at the same time. Nor can they be completely defined in terms of any number of events or existences that are presented in experience full-fledged (so to speak) at any given time, or, for that matter, at any number of times. For there are always inactive, undeveloped or unexpressed capacities for the determination of such events or existences, which are not those events or existences, but which may determine them at some future time. Such capacities, powers or possibilities must be allowed for in our definitions, if we are not to ignore a large and significant portion of the facts of existence.

And we know that not all such capacities have been discovered for these various sorts of entities, and probably not for any one of them. A great part of scientific research is directed to the discovery of hitherto unidentified capacities in the various sorts of entities that.

taken together, constitute nature. Adequate definitions must allow for these possible undiscovered capacities also.

It may be pointed out that we act in every field of human enterprise on the assumption that we have to do therein with numerous diverse capacities or possibilities that are not expressed in experience at the given time, whether sensory or of other sorts. In training the young, for example, we are all the time working, as we assume, with their undeveloped capacities, stimulating some and directing them along definite lines, while repressing or subordinating other capacities. To give a concrete illustration, the acquisition of innumerable habits by the human individual is wholly unintelligible save on the assumption that at birth the baby is endowed with a relatively high capacity for habit-formation, a capacity which is quite plastic, as we say, and capable of being directed along any one of innumerable diverse lines of development. The chemist, the physicist, the geneticist, the farmer, the manufacturer, the physician, the statesman are likewise dealing continuously with such undeveloped or inactive capacities, so manipulating them as to evoke developments deemed desirable by these various workers.

These seem to us at least to be obvious facts. And we do not see that the basic position of positivism can be made to square with them, except, as in other cases, by relegating them to the status of possible facts, or of useful conceptions whereby to describe groupings and sequences of sense-impressions. But, not hoping to convince the positivist that his position on this matter stands in need of revision, we offer him all these capacities, powers, potentialities, for what they may be worth to him, whether as possible facts or as useful conceptions.

We are mainly concerned, however, to point out that the various types of entities deduced by our analysis are conceived in terms of such capacities and possibilities as well as of the phenomena, the events, the full-fledged existences in which these capacities and possibilities may realize themselves. We shall leave the matter in this form, without elaborating the position in relation to other types of ontological doctrine which would be critical of it. Its critical implications for those types and theirs for it can be readily identified and assessed by those who may be interested.

Further Defense of Methods Employed in the Analysis

We shall now elaborate somewhat further our defense of methods employed in the analysis, and point out some implications of the latter for future scientific work in this and other fields.

The legitimacy of the methods herein employed would appear to depend ultimately on the validity of the logical principles considered in the first part of this chapter, as the analysis represents a systematic application of those principles. If the applications have been accurate, the legitimacy of the methods employed can be successfully impugned only by showing that the underlying principles are not valid ones. Since, so far as we know, the validity of those principles is not questioned, we may assume that they constitute adequate logical justification for the methods employed in the analysis. Criticism must therefore, on this supposition, be limited to the specific applications of the principles in question, including the deductions therefrom of the several methods emloyed in the inquiry. It is of course to be expected that so complicated an analysis as ours will present many occasions for criticisms of this sort.

The application of those principles and methods to our special problems has involved a systematic analysis of the type usually termed logical or inferential; and this analysis has yielded conclusions both scientific and metaphysical in character, according to the ordinary connotations of these terms. The defense of such analysis previously undertaken [1] need not be recapitulated here. We contended, it will be recalled, that this type of analysis is indispensable to the proper treatment of the problems dealt with in the present inquiry, and indeed of all complex scientific problems. It was shown that only through such analysis can we determine the legitimacy of specific scientific problems, the implications of farreaching scientific hypotheses, the validity of the broader scientific categories, and the adequacy of particular procedures of investigation, such as have been considered in our inquiry. And through an analysis of this type we could demonstrate, in our special field, that many problems are artificial and insoluble, because of defective formulation; that a number of hypotheses are untenable and even self-contradictory, because of hitherto unrecognized implications; that various categories are seriously defective, because of faulty classificatory bases; that certain investigational procedures are inadequate, because of misconceptions respecting the functions of experimental and other scientific methods. On its positive side, the analysis, besides offering a justification of the methods employed, yielded most important conclusions on the problems treated by the analysis.

There are those, however, who would concede the legitimacy and even the necessity of the systematic analysis of scientific hypotheses, such as we have undertaken for the problems covered by our inquiry;

[1] See pp. 173–179, 368–369, 382–384.

but who would at the same time contend that such analysis does not serve to *establish* definitive conclusions respecting the problems thus treated. It is held by those taking this position that new hypotheses developed by such analysis can be tested only by fresh evidence, evidence yielded by observations or experiments specially designed to verify or refute these hypotheses. This is the position taken by a contemporary school of logicians, who maintain that a complete act of thought includes as its final stage the testing of hypotheses respecting the problem under consideration, by applying them to one or more concrete situations of the sort to which they have reference. A negative implication of this doctrine is that facts may be conceived which would refute any given hypothesis, and that the definitive appraisal of such hypothesis involves the search for facts of this character, and the test of the hypothesis by any such facts that may be discovered.

As with many other theories, the grounds for our position on the problems emphasized by this particular theory must be briefly indicated, and without attempting a systematic examination of the theory on its own account. We shall say at once that with the spirit of this doctrine we are in complete sympathy. Hypotheses must be tested by the relevant facts, and any hypothesis that cannot be tested by fact is without scientific value, though, as we have had occasion to point out, valuable hypotheses are often entertained for considerable periods of time before they can be put to the test of fact. This criterion of the legitimacy of hypotheses we have attempted to observe and apply throughout our inquiry.

We should be more critical of other features of the doctrine under consideration. For one thing, we do not believe that the testing of scientific hypotheses depends in all instances on the collection of additional empirical data for this special purpose. Existing data may when properly evaluated serve such a purpose. And available data when thus evaluated may be superior as test evidence to unavailable data which might by some be deemed essential to a settlement of the problems at issue. We contended that this is true with respect to the questions in controversy between Lamarckians and anti-Lamarckians. We have contended, further, that the settlement of difficult scientific questions often depends on the systematic analysis of empirical data, rather than on the accumulation of further data; and that many such problems cannot be settled by any amount of empirical data unless treated in this way. We have assumed that conclusions on many of the problems treated by us would be yielded by a systematic analysis of data relevant thereto, which are now available. We did not, however, fail to point out a good many specific problems in

our field which can be solved only through the collection of fresh data. And other problems of this character are to be indicated in a later part of the present chapter.

The view that scientific investigations must all terminate in the examination of concrete facts bearing on the several hypotheses under consideration has been considered, by implication at least, already. We should say that systematic investigation must in all its stages have reference to the classes of facts with which it is concerned. It does not merely *terminate* in the examination of such facts. It is an examination of the relevant facts throughout. There must of course be sufficient facts of the right sort known, if definitive conclusions on the problems under examination shall be possible. But the already known facts may be sufficient, when properly evaluated, to yield such conclusions. The final work involved in reaching conclusions of this character may be undertaken by experimental scientists or by theoretical students of the given problems, depending on the stage at which the consideration of the given problems has arrived; and may therefore variously emphasize experimental research bearing on such problems, or logical analysis of the already available empirical evidence thereon. In either case, empirical data *and* logical analysis (or systematic inference) are necessary to *any* conclusions, be they right or wrong ones.

It would appear to be an error, therefore, to hold that the logical analysis of hypotheses may not yield definitive conclusions on the problems to which they refer, unless this be divorced (if such a thing is possible) from the data to which the problems and the hypotheses pertain. Logical analysis of hypotheses and evaluation of data bearing thereon are, in reality, inseparable. Our own analysis of hypotheses, we submit, has had reference to the relevant empirical data throughout. And where we advanced hypotheses of our own, we attempted to show, as best we could, that only those hypotheses, and none of the opposing hypotheses, fitted the relevant facts.

Finally, we were careful to consider evidence that appeared on its face to be incompatible with the hypotheses advanced by us. And as the analysis proceeded, the hypotheses advocated were developed or qualified to conform to the evidence successively introduced, when this was found to be necessary. The aim, in short, was, not to force conclusions on the facts, but to let the facts, properly estimated, force their own conclusions.

There may of course be facts not known to us or to others, which will necessitate a revision or abandonment of many of our conclusions. We are inclined to think, however, that a sufficiently wide range of

representative facts has been considered to justify our conclusions, so far as bare empirical data could do so. Correction or refutation of our conclusions seems to us to depend more, therefore, on the critical appraisal of our estimation of the evidence dealt with, than on the testing thereof by additional empirical evidence. It is certain, however, that much of the relevant evidence has not been considered by us, and we may expect disproof or emendations of many of our conclusions through the utilization of such evidence.

It would be easy to imagine species of empirical evidence which would contravene the various hypotheses we have advanced. For example, if it could be shown that any species of organism possesses a hereditary active adaptation to some specific, and especially a unique and irreproducible, feature of the environment, without that condition having influenced the ancestors of the given species, the correctness of our analysis of this class of characters would be rendered very doubtful, though it is difficult to see that such evidence would render any of the opposed hypotheses more tenable than ours. Again, the discovery of animals whose action with reference to distant objects was demonstrably determined by electromagnetic or other forms of energy that bridged the gap between them would necessitate a modication of our conclusion that such action is determined by non-physical as well as physical factors. Also, if it could be shown that chemical and physical processes in inorganic systems are sometimes interrelated according to quantitative, qualitative, spatial and temporal specifications, like those characterizing the organization of such processes in the organism, our conclusion that organizatory factors are involved in physiological activities would have to be abandoned or modified.

We offer these illustrative suggestions in order to satisfy, if possible, those methodologists who would demand that possible empirical data which might refute our hypotheses should be specified. As aforesaid, however, refutations and corrections of our conclusions must in our judgment come for the most part from a superior logical evaluation of the type of evidence that we have adduced in support of these conclusions.

Some Practical Implications of the Analysis

It may be pointed out that the analysis as a whole carries far-reaching implications for all departments of science and philosophy. It strongly supports the proposition that science and philosophy are absolutely interdependent, and that it is fatal to attempt their separation. Science when it becomes critical and comprehensive *is* philosophy as

well as science, and philosophy that is worth the name must contribute to the solution of scientific questions. For the more fundamental problems of science must be based on sound metaphysical assumptions, if they are to be soluble, and solved, if at all, by data evaluated in the light of critically formulated logical principles; metaphysical speculations and logical principles must, in turn, be tested, corrected and elaborated by just this process of solving fundamental problems common to science and philosophy.

These truths carry with them a condemnation of the attitude all too prevalent among scientists that philosophy has nothing to do with scientific research. This attitude comes out in the scientist's failure to recognize the hypothetical character of the basic assumptions upon which he works; in his conversion (albeit unconscious) of these assumptions into metaphysical dogmas standing for infallible truth; in his open contempt for scientists who question these assumptions and the truths for which they are believed to stand; in his unfamiliarity with and even indifference toward facts in other fields which bear on his own problems; in his misconception of the functions both of experiment and of logical analysis, in the treatment of scientific questions. These attitudes are due, of course, to downright ignorance, and to the illiberality of mind that goes with it. These in turn are due to specialism in training and in experience, as well as, it must be confessed, to a limited interest and imagination that prevent the scientist from seeing beyond his specialty to the larger background of reality in which it should be set. The result is that in a field where we should expect the outlook to be broad, the imagination active, and authority as such without influence, we find progress hampered by dogma, the outlook on reality limited, activity routine and sterile to a degree.

The professional philosophers are open to much the same criticisms. Too often they have been indifferent to the aims of science, and failed to recognize the necessity of coöperating with science in the treatment of its fundamental problems, or the possibility of accelerating the progress of philosophy itself through the utilization of scientific data bearing on its problems. All this comes out in the view taken by some philosophers that philosophy is the study of its own history; in the attempts often made sharply to distinguish the sphere of philosophy from the sphere of science; in the failure of many philosophers to employ the results and methods of science in dealing with problems supposed to be in their exclusive sphere; in the popularity among philosophers of the doctrine that the problems of science must be dealt with through observation and experiment alone (as if such a thing

were possible); and indeed in the tendency, sometimes observable, to erect that dogma into a sort of scholastic experimentalism conceived to be valid anywhere and everywhere, as least potentially.

The outcome of such attitudes is that we are cursed by specialism in every field of intellectual endeavor; problems persist which might be solved, were there more give-and-take between specialized intellectual disciplines; and our intellectual interests are so isolated and inbred that they do not contribute to the progress of society or to the development of human personality as much as they might contribute. The greatest danger confronting intellectual interests to-day lies in the tendencies making for still greater specialism in scientific research, and a still wider chasm between science and philosophy. By the same token the greatest opportunity now open for contributing to intellectual progress lies in the direction of checking this specialism, and healing this schism. For that reason the work of men like Bergson, Driesch, J. S. Haldane, William James, Edmund Montgomery and others that could be mentioned possesses a special significance. For they have demonstrated by their works that coöperation between science and philosophy is essential to the best progress in the treatment of fundamental problems. They and others have demonstrated that cooperation between the sciences themselves contributes and is indeed indispensable to scientific progress. Fortunately, the importance of coöperation between the sciences is more widely recognized, as the establishment of *synthetizing* sciences, such as physical chemistry, physiological psychology and sociology, demonstrates.

Many additional implications are contained in the specific results yielded by the analysis. The practical import of such implications is of course conditional on the validity of these results. Assuming the latter, the more important of those implications may be briefly indicated.

By far the most important is that we should have a synthetic theory of the primary factors in life and its evolution, upon which to base future work. If this theory be valid, the effort hitherto expended in controversies on this subject might be largely devoted to problems of a different and more specific character. We should, for example, cease debating the question whether functional activity is a primary factor in organic evolution, and seek to determine, instead, the specific rôles played in hereditary changes by various types of functional activity in different species of organisms. The possibilities of this type of investigation for plant and animal breeding, the improvement of mental and physical traits in the human species, and other practical biological interests are obvious. We should also cease

debating the question whether organic activity is exclusively physico-chemical in character, and attempt to determine instead just what rôles specific physical and chemical factors play in the economy of the organism, to what extent these might be controlled with a view to furthering practical interests affected thereby, and the like. Similarly the specific rôles of organizatory factors in evolution and development, together with the possibilities of controlling these factors in desirable ways, would constitute another group of problems in the general program here set forth.

Under the new régime, the claim of psychology to be a branch of science coördinate in dignity and importance with physics and chemistry would no longer be disputed, nor the necessity of distinctive methods for dealing with mental, or sub-mental, factors. Behavioristic metaphysics of the type represented by Watson and others would disappear, though behavioristic methods would still be employed in psychological research. But mental processes as such would be studied by the only method whereby they appear to be accessible to us, that is, by introspection.

It seems not impossible that a special science of organizatory factors will be established, coördinate in importance with biological chemistry, because dealing with a category of factors equal in importance to chemical factors. The *physical* processes in the living organism may eventually constitute the subject matter of a special biological science. Already there are tendencies in the direction of such a development, as investigations in the physical chemistry of vital phenomena, the establishment of colloid chemistry as an independent science, and other scientific movements indicate. Psychical research, so-called, may eventually assume great importance, from the standpoint of the biological sciences, purporting, as it does, to deal with one important group of factors operative in the human organism, after dissociation from the organism. The biological significance of cultural factors, especially for the human species, must also be a topic of systematic research, after its importance shall have been realized. Ethnology and other sciences dealing with these factors in human society must actively coöperate with the biological sciences in investigating this group of factors in relation to evolution and development, or a special science for this purpose be established.

We have here some very plain intimations as to the necessity of other than biological sciences coöperating in future work on the theory of evolution. The development of this theory can no longer be regarded as the exclusive prerogative of the biological sciences, but all other sciences dealing with living organisms should be expected to co-

operate in the further development of the theory. This means physics, psychology and sociology, in particular, as well as special sciences that may be established for dealing with specific categories of factors in vital phenomena. There must be the freest give-and-take among all these sciences, as well as between them and philosophy, if the best progress in developing the theory of life and its evolution is to be assured.

Assuming that the categories of factors operating in vital phenomena have been correctly identified by us, future work in this field should be largely concentrated on the analysis of specific factors falling within these several categories. Sharper distinctions must be made in future work between *descriptive* categories, such as selection, adaptation, reproduction, mutation, genes and the like, on the one hand, and *factorial* categories, such as chemical reactions, physical processes, organizatory, mental and cultural factors, on the other hand. Descriptive categories must still be used, of course, because the biological sciences will continue in their descriptive stage of development for some time to come, and because, also, the structure and activity produced by the various factors at work in the organism must always be dealt with in descriptive terms. However, future biological research will have as its ultimate aim the analysis, synthesis and control of the factors which determine vital activity. *Pro tanto,* there will be an increasingly smaller emphasis on and use of purely descriptive categories.

Finally, biological work in the future will involve a more extensive use of systematic analysis than has characterized it in the past. It will be recognized that experiment and observation are only particular types of procedure in a more elaborate technique of scientific inquiry, and not a complete substitute for the latter. It may be that, owing to differences in type of interest and to the necessity of specialization, a special discipline, at once a branch of biology and of philosophy, must be established, whose function it will be to criticize and amend the assumptions, problems and methods of experimental biology, while accepting from the latter materials for the use and enrichment of logic, metaphysics and other departments of philosophy.

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The author of this dissertation was born in the southern part of Johnston County, North Carolina, July 22, 1885. He attended the public school of that district, 1890–1900; Buies' Creek Academy, 1900–1901; North Carolina State College, 1904–1907; and Columbia University, 1908–1912 and 1916–1918. He received the degree of Bachelor of Engineering from North Carolina State College in 1907; and the degree of Bachelor of Arts from Columbia University in 1911.

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